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ITER: Too Large to Stop and Too Large to Continue?

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Contents

Abstract

What are the forces of growth that have made the science of fusion evolve into the research project ITER, and how may they be accounted for with regards to the particular challenges to which the project has been subjected during the last two decades? This problem is intended to shed some light on the underlying causes for the difficulties faced in modern fusion science. Currently, ITER is the largest venture ever devised in fusion and the very pinnacle of over 60 years of global research. Its primary objective is the scientific and technical demonstration of fusion as a practicable method for future large-scale energy production. Despite several plus points, developing fusion through ITER for energy purposes is not devoid of complications; if truth be told, its benefits are more or less offset by its challenges. I maintain that the source of these challenges is that ITER has become too costly, and will possibly remain so regardless of how its challenges are dealt with. Indeed, the cornerstone of the excessive expenditures is the project's accumulated size which is primarily dictated by the particular reactor technology it utilises, called *tokamak*. Paradoxically, this very size is what makes the project push forward. Likely explanations for this situation are found in a literature study on fusion research history. This is conducted firstly by defining the history in terms of three variables: the international fusion community, the political framework, and the fusion reactor technology itself. Secondly, these are organised into four decisive phases and then framed, through theoretical discussions concerning the concept of Big Science, as inherent mechanisms of growth; mechanisms that have collectively facilitated the scientific progress in fusion. Finally, the actual functions and interrelatedness of these mechanisms are clarified through perspectives on path dependency, deriving from evolutionary economics. The objective here is to demonstrate that the evolution of science can apply well to growth patterns that are comparable to those of markets and large technological systems – indicating science as a concrete system of its own. Accordingly, I am suggesting that fusion research is a system of science that has culminated in the ITER project after years of accruing improvements and momentum. Ironically, this momentum has made ITER become detrimentally large and inflexible over time owing to its profound contingency on the high-demanding tokamak technology.

Chapter 1. Background

1.1. Introduction

What are the forces of growth that have made fusion research evolve into ITER, and how may they be accounted for with regards to the particular challenges to which the project has been subjected during the last two decades? Based on a literature study, this is the research problem I am using in order to give details on the present difficulties faced in fusion science and its ostensible Big Science properties. ITER (International Thermonuclear Experimental Reactor) is a joint effort represented by seven partners, featuring the People's Republic of China, the European Union, the Republic of India, Japan, the Republic of Korea, the Russian Federation, and the United States. It is currently the largest project ever orchestrated within the field of controlled thermonuclear fusion (usually abbreviated as 'CNF' or simply 'fusion'), denoting the pinnacle of over 60 years of global research. ITER's primary objective is the scientific and technical demonstration of fusion as a practicable method for prospective large-scale energy production, a method involving the generation and utilisation of an extremely hot discharge called *plasma*. Such operations are dependent on an advanced reactor technology that is yet to be built and perfected; a venture to which ITER seems the most promising approach. ITER's assignment has harnessed broad legitimacy with fusion being regarded as the only realistic alternative to today's fossil fuel regime, owing to its capacity for scale (unlike intermittent technologies such as wind and solar power). Indeed, fusion has the potential of producing an annual energy amount that is ample for meeting the demands of a continually growing world population. This has to do with the abundance of its fuel which is to be extracted from regular seawater. Furthermore, the energy production itself will be conducted in a virtually fail-safe procedure that is entirely free of hydrocarbon emissions. If successful, ITER will greatly help to realise the almost utopian prospect of commercially viable fusion power, adapted for full-scale plant installations. As of 2010, ITER has recently commenced the preliminary stage of constructing its multibillion dollar test reactor site at Cadarache, France, following 25 years of preparatory studies, project planning, design work, and organising.

Apart from its instrumental purpose, ITER itself is a subject of various interests that can be studied from scientific, social, political, and economic viewpoints, to name a few. For instance, the project is a considerable advantage to several areas of basic science because the international endorsement it receives is unmatched for research at such an experimental level. This pertains to legitimacy, funding, number of jobs, etc., and the fact that ITER is set to be operational for a minimum of 30 years. Related to this is the notion of ITER being a conduit for transnational knowledge distribution, and it is frequently stated that one of its most valued rewards is the learning gained from the project's pooling of worldwide expertise. Politically, the collaborative aspect was even held as a weighty incentive for ITER's conception in 1985, with the vision being a vehicle of Big Science to unite the Eastern and Western blocs towards a common goal. With regards to macroeconomics, ITER is an agent of a financially expansive governmental investment which aims to foster an alternative energy regime that might replace the petroleum economy. In a shorter term, ITER is of notable importance to industry seeing as its requirement for technologies which include cryogenics, superconducting electromagnets, robotics, heat- and radiation-resistant materials, etc., is a substantial stimulus for technological innovation. This is on account of the extreme character of the ITER reactor specifications that command heavy upgrades and customisations of its necessary components. Some components were not even invented at the time of the reactor's main design activities – and some are still being worked on today.

Despite the evident plus points of fusion, the prospect of using it for large-scale energy production is not devoid of complications. If truth be told, its benefits are more or less offset by its challenges, and it is this reality that I am interested in investigating. Cost-effectiveness, for instance, is likely to be possible only if future fusion power plants are installed to serve a substantial number of users due to the expensive technology it is expected to employ. This is the tokamak[1] reactor concept which is the centre of ITER; a technology heavily dependent on economies of scale. Furthermore, it is plausible to think that given a fusion-based replacement for fossil fuels, one might see an additional shift in the structure of energy systems and policy through increased centralisation with fewer power plants per city or country. Such an outcome opposes the idea of practicability which is important for worldwide application, and in point of fact, tokamak fusion is already indicating monopolistic qualities that, if implemented, will have need for extensive governmental regulation. With this in mind, let us move on to ITER's more current challenges to find out what may imply such an outlook for fusion energy.

In the course of the last 15 years, the ITER project has been subjected to quite a few obstacles, including deficits, machine downscaling, project re-organising and re-planning, and repeated delays. The most recent predicament is related to segments of the construction phase.

[1] Russian acronym for ***to****roidal'naya* ***ka****mera v* ***ma****gnitnykh* ***k****atushkakh* meaning 'toroidal chamber with magnetic coils'.

As agreed, ITER will be built, operated, and managed by all partners, and thus each partner is responsible for delivering specific components for construction. This is organised through in-kind contributions involving components for the ITER machine itself as well as auxiliary test site systems. The EU partners, however, are reluctant towards initiating full construction of their components without ensuring that these are working properly, and have thus stated a wish to make prototypes for pre-emptive scrutiny. This is problematic seeing as France (under the EU) is hosting the ITER test site and is responsible for installing infrastructure and other facilities that need to be functional before the actual reactor and its associated systems can be built. After several delays, test site completion has been set for 2018; a deadline that does not allow sufficient time for prototype checking *prior to* site construction, meaning that the EU partners may only conduct these activities in parallel. Such a compromise is pointless because potential component flaws and deficiencies that may be uncovered during prototype tests are likely to require alterations to building designs and alike. Ultimately, this will incur additional project costs should the construction work on these facilities already be in progress. The EU partners are therefore negotiating to further delay the project deadline (current discussions are looking at November 2019) while the other partners are determined to continue according to the present schedule: there is little doubt that added hold-ups will elevate project costs due to working, but idling, in-kind contracts with domestic industries. What is more, the urgency of climate issues as well as peak oil[2] estimates are also being stressed as incitements for pushing forward with ITER.

While the EU risk aversion dilemma and other issues are being thoroughly attended to, my understanding is that no one seems to contemplate the project's underlying problem: that ITER has become too costly, and will possibly remain so regardless of how its challenges are dealt with. I recognise that this may be a rather explicit assumption, but I also think that the assumption reflects a rather explicit situation. Indeed, the cornerstone of the excessive level of expenditures, I argue, is the accumulated size of ITER which is predominantly dictated by the particular reactor technology it utilises. Paradoxically, this very size is what makes the project carry on. For this reason, my research problem is intended to show why ITER is both the most prestigious undertaking and the most acute impediment in fusion based on the probabilities of how tokamak science and technology have been enabled to grow.

[2] Peak oil is expected at around 2020; an event that fusion power may capitalise on as a prospective option to the petroleum economy. Hence, a fully operational ITER at the occurrence of peak oil would be beneficial in this respect.

1.2. Literature and methodology

The data material used in this literature study is primarily retrieved from books that offer different accounts on the history of fusion research. This is to obtain a comprehensive idea of how long-standing preconditions might have enabled fusion to advance into its position with the ITER project, and how these may affect the project's current complications. In addition, I have relied on articles, papers, reports, and websites. These work either as supplements to the books' historical contents or as separate sources covering the more recent progress in fusion that are either lacking or omitted in the books. This includes in-depth technical information on reactor-related topics and fusion research in general, discussions on economic contingencies, but first and foremost ITER-specific information. I should point out that both theoretical and technical details are tightly linked to the historical proceedings of fusion, and in the various data that I have come across they are typically expressed with mathematical models, numbers, and figures that offer poor accessibility or significance to a social study like mine. Therefore, the qualitative findings provided by the history literature have worked as valuable references for interpreting quantitative information.

The history literature that covers the first 20-30 years of fusion research has a shared thematic composition. It contains descriptions of initial research activities, followed by the dissolution of national research secrecy, the international expansion of the scientific fusion community, the 1969 breakthrough of the tokamak, and the ensuing scientific progress with ever larger and ever more sophisticated reactor designs. An implication of this self-appointed 'mainstream' perspective is found in the authors' emphasis on magnetic confinement as the foremost method of creating controlled fusion reactions. This is most notably represented by the tokamak concept. The evolution of less prominent fusion approaches, e.g. pinch devices, 'cold fusion', and laser inertial confinement systems, is more moderately discussed. This is because they make up alternative paths to fusion energy that have either been inferior to the tokamak or too immature to serve as candidates for ITER. In other words, the literature I have made use of deals with the first and, still, only potential success story in the science of fusion.

With that being said, the literature's perspectives do differ to some extent with respect to the technical, social, and political issues surrounding magnetic fusion research, much due to the authors' individual background and stance. T. Kenneth Fowler's "The Fusion Quest" (1997) is comparably narrow in its scope as it revolves around the basic principles of plasma physics and fusion reactions, and the technical challenges they have instigated particularly for

American physicists and engineers to develop a fusion reactor scheme. Fowler himself is a pro-fusion nuclear physicist. He has both taken part in American fusion research during the 1960s, 1970s, and 1980s, and in the organisation of ITER's preliminary design groups in the late 1980s. His first-hand experience thus provides a valuable insight for describing portions of the scientific development from an American physicist's position[3]. Lisa Bromberg's work in "Fusion: Science, Politics, and the Invention of a New Energy Source" (1982), on the other hand, was commissioned by the leaders of the American programme on magnetic fusion energy with the purpose of documenting their own history in a coherent and unbiased manner. Bromberg, a historian, presents second-hand information derived from literature and archive studies, in addition to interviews. The book contains much of the same topics and events as Fowler, although with less technical emphasis when explaining the various reactor approaches and configurations. Instead, she devotes greater focus on fusion's organisational and political circumstances. A more inclusive perspective is employed in "Fusion: The Search for Endless Energy" by journalist Robin Herman (1990). Here, the international and collaborative nature of fusion research is particularly highlighted, thus directing attention as much outside the American fusion community as inside. The Soviet research effort is widely addressed in this context. Like Bromberg, Herman's work is largely based on interviews with scientists being or having been involved in fusion research. The fourth book, "Nuclear Fusion: Half a Century of Magnetic Confinement Fusion Research" is written by C. M. Braams and P. E. Stott, the former a retired professor of plasma physics, the latter a scientist with experience from several renowned fusion laboratories and later the Euratom-CEA Fusion Association. Their scope is similar to Herman in that it addresses the history of fusion research on a more global basis than Fowler and Bromberg. Then again, it resembles Fowler due to its fairly technical profile, especially when discussing the early fusion technologies, the different tokamak variants that emerged during the 1970s and 1980s, and the ITER tokamak specifications. The book also takes a clearly normative turn, stating that the success of fusion depends on that society makes the "right decisions". Finally, the article "The initial period in the history of nuclear fusion research at the Kurchatov Institute" by Russian nuclear physicist V. D. Shafranov gives a brief but sensible account of the birth of fusion and its sophomore period from the Soviet point of view. Like Fowler, Shafranov writes on the basis of personal experience.

Apart from the structural similarities, the majority of the literature material reveals an overall discussion, featuring an overall problem: the utopian prospect of viable fusion power

[3] Similarly, the 2001 article by V. D. Shafranov, "The Initial Period in the History of Nuclear Fusion Research at the Kurchatov Institute", gives first-hand accounts from a Soviet/Russian point of view.

and the scientific, technical, political, and economic trials associated with its realisation. This rather bittersweet subject is reflected in the fact that fusion research, albeit a potential godsend to future energy, has always been an extremely advanced and fairly unfamiliar terrain within the field of nuclear physics; one that has revealed recurring setbacks. In the face of adversity, though, the history literature indicates numerous events that have helped fusion traverse these setbacks and move forward with increasing pace and authority. My position in this context is sympathetic given that I, too, am attending to both the progression and the various challenges in fusion science: those of contemporary impact being confronted by ITER. However, what I wish to add to the discussion is that fusion has prospered apparently from a process of natural selection, growth, and momentum, resulting in the technological trajectory of the tokamak. Linked to this, while fusion has managed to overcome many and formidable trials thus far, its past, or more accurately *path dependency*, may be catching up with it; triggering even greater trials which I argue are observable with ITER – literally as a ghost in the machine.

What is more, corresponding to this positioning is also the affirmation of my literature study methodology, namely *interpretation*. By using the featured literature and interpreting it, I have been able to organise the history of fusion into an analytically manageable object. This methodology is equally pertinent to the theoretical instruments (presented in the next chapter) with which I am analysing the object. In this way, I have achieved an understanding of the dynamics that arguably have propelled the science of fusion to its current state and on which I have drawn to carry out my study of ITER in view of its long term developmental path.

1.3. Controlled thermonuclear fusion (CNF)

Before presenting ITER, I shall give a brief explanation of the scientific objectives ubiquitous in all fusion history to establish a basic understanding of the subject. The central objective of fusion energy in the context of confinement technologies translates to the rather self-evident purpose of designing and building a device that produces more energy than it consumes, i.e. energy breakeven and higher. This feat is to be achieved by generating and controlling an enormously hot discharge called plasma, which is also known as the fourth state of matter following solid, liquid, and gaseous. The procedure of making and fuelling plasma involves fusing ("forcing together") two light elements (deuterium and tritium) extracted from ordinary seawater. The ambition of fusion as an energy source, however, is a sizeable one as it relies on a number of stringent physical preconditions pointing to the term 'Lawson criterion', as originally derived by John D. Lawson in 1955 and published in 1957 (Lawson 1957, p. 1). The Lawson criterion is basically an energy principle which evaluates the ability of magnetic field configurations to confine plasma discharges in a stable condition, and is therefore the main reference in fusion reactor theory (Fowler 1997, p. 215-17). It is defined by the so-called 'triple product', i.e. a mathematical function of *plasma density*, *temperature*, and *confinement time* which are the critical parameters of a fusion reaction. Once energy breakeven is reached, the second fusion objective is to obtain 'ignition'. *Ignition* indicates that plasma heat is sufficient (150 million degrees) for maintaining its temperature without external input. This means that the plasma is self-sustaining, i.e. enabled to heat itself. In a power plant, this massive heat will be converted into electricity by conventional means (e.g. steam turbines). Techniques for fulfilling the three criterion parameters both individually and collectively have been developed and tested in a variety of reactor concepts ever since the birth of fusion research.

1.4. ITER – International Thermonuclear Experimental Reactor

1.4.1. The project

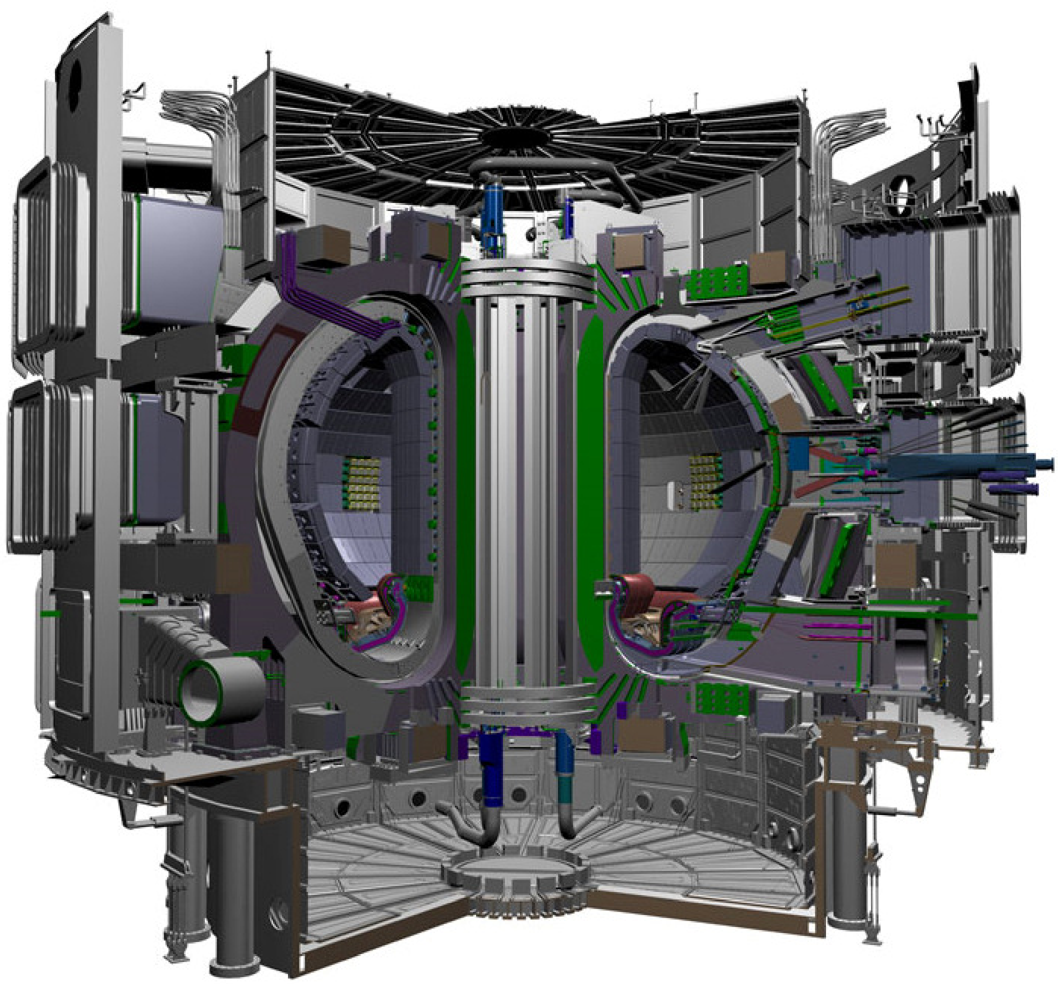
The ITER project was set up in 1985 as a collaborative agreement between the United States, the EU (through EURATOM), Japan, and the Soviet Union. Ever since, it has denoted the pinnacle of over 60 years of fusion science, aiming primarily to demonstrate the feasibility of fusion as an energy source through a 20-year operational period of experimental research that follows 10 years of construction. ITER's scientific work will be centred on a highly advanced machine that represents a new generation of fusion reactor technology. Secondly, during these years, the project is also expected to map the requirements for building and operating a full-scale electricity-producing power plant that will serve as a prototype for future commercial plants.[4] When finally built, ITER will be the biggest project in the history of fusion not only on paper but in real life. Moreover, it will be the world's most expensive scientific endeavour after the International Space Station (ISS) (Clery 2006[5]).

[4] http://www.iter.org/proj/Pages/Default.aspx - 03.03.2010
[5] http://news.sciencemag.org/sciencenow/2006/11/21-01.html - 06.04.2010

1.4.2. The machine

The ITER machine is a tokamak test reactor which will be the most massive of its kind when construction ends somewhere between 2018 and 2019[6].

Figure 1. Cross section of the internal systems of the ITER tokamak (source: www.iter.org – 05.12.2009)



[6] As of 30.03.2010.

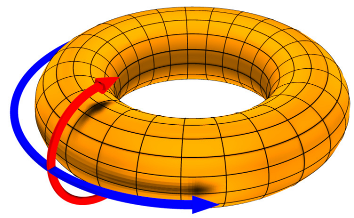
Throughout its evolution, the tokamak has been engineered in many different versions, and the machine assembled as ITER will bear minimal likeness to the earliest models. Even so, all tokamak versions derive from the same principles and physical preconditions; the same unique characteristics that made the tokamak the superior approach to fusion over 40 years ago. Being a complex network of hardware and software that incorporates virtually as much technology outside the machine core as inside it, ITER's various components can be grouped as internal and external systems. I will, however, concentrate this summary presentation on the machine's internal systems as they comprise the main architecture of the ITER test reactor, and hence the configuration that distinguishes the tokamak approach from other fusion concepts (external systems will be referred to in the Big Science section). The systems are listed as follows:

- Magnets
- Vacuum vessel
- Blanket
- Divertor
- Diagnostics
- External heating
- Cryostat

The system of *magnets* has a cage-like design and is primarily made up of 18 *toroidal field* coils (TF coils), 6 *poloidal field* coils (PF coils) (see figure 2 for explanation on geometry), and a set of correction coils that enfold the hub of the machine, plus a central *solenoid* coil. The system serves to both heat and regulate plasma currents by confining and shaping them via the extremely powerful magnetic fields it generates. The magnets are run by electricity, and in order to make them as efficient as possible in terms of electricity consumption, they are designed with superconductive properties. This means that they contain composite materials that lose resistance, and thus increase electrical conductivity, when being cooled to cryogenic temperatures, i.e. in the range of -269 degrees. Additionally, the central solenoid also works as a transformer that produces the main electrical current that sets the stream of plasma in motion.[7]

[7] http://www.iter.org/mach/Pages/Magnets.aspx - 03.03.2010

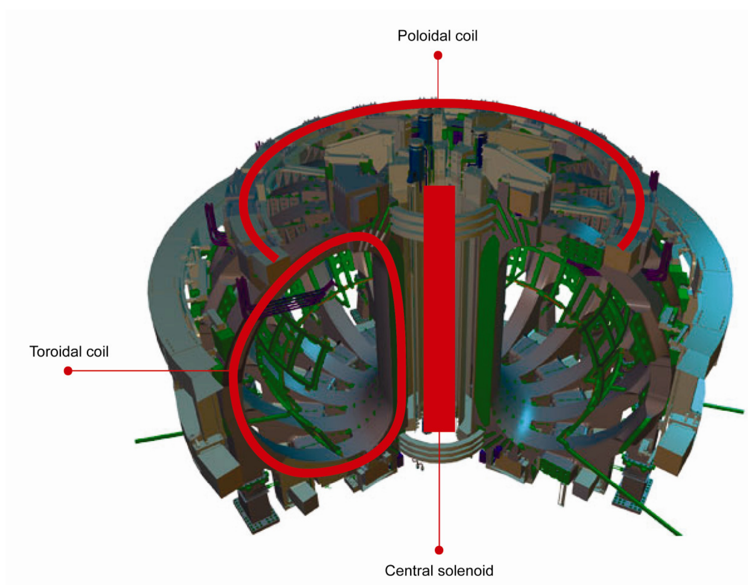
Figure 2. Field geometry (source: http://en.wikipedia.org/wiki/File:Toroidal_coord.png – 18.10.2009)



Note: Toroidal direction field (red arrow) and poloidal direction field (blue arrow) in torus (yellow figure) (Burke 2006).

The *vacuum vessel* is a double steel-walled chamber wherein the actual fusion reaction is contained. Here, the continuous stream of plasma is revolving in mid-air. The name implies that the component is hermetically sealed and works as a first safety containment barrier that separates the plasma from the cryogenic conditions outside the vacuum vessel. The amount of energy produced by a tokamak device is determined by the dimensions of the vacuum vessel as larger vessels allow greater plasma volume and, thus, more power.[8]

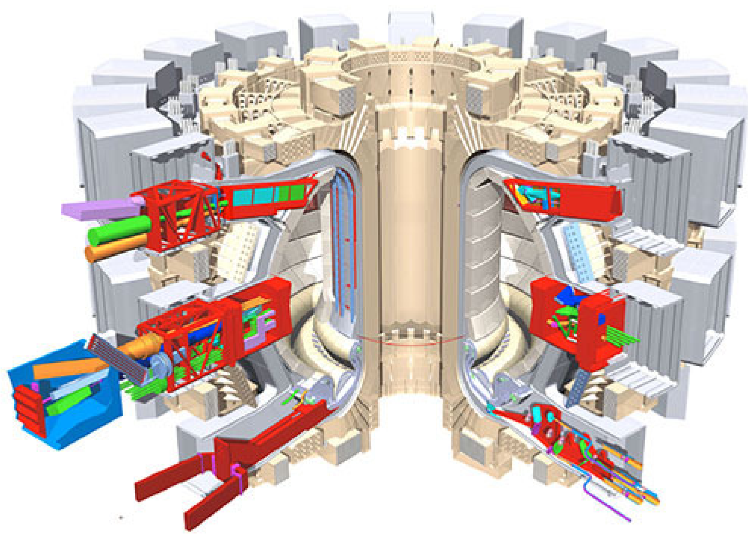
Figure 3. Cross section of ITER magnet system (source: www.iter.org – 05.12.2009)



[8] http://www.iter.org/mach/Pages/VacuumVessel.aspx - 03.03.2010

The *blanket* is a modular (replaceable) system which is placed on the interior surfaces of the vacuum vessel. It consists of 440 individual segments, each with a removable plasma-facing front wall. Its purpose is to provide shielding to the vessel and the magnets from the heat and radiation (neutrons) of the fusion reaction. The blanket modules convert the kinetic energy of the neutrons into heat energy which, in a fully functional fusion power plant, will be used for generating electricity.[9]

Figure 4. Cross section of ITER diagnostics (source: www.iter.org – 05.12.2009)



The *divertor* is a complex exhaust system that consists of 54 remotely-removable cassettes that are located in a row at the base of the vacuum vessel. Since the fusion reaction produces impurities within the machine that may compromise the operation, the divertor is designed to extract these impurities from the plasma.[10] The ITER tokamak also features around 50 individual *diagnostic* systems which are installed for controlling, analysing and optimising the performance of the plasma. This involves lasers, x-rays, neutron cameras, impurity monitors, pressure and gas analysis, etc.; instruments made for studying temperature, plasma density, impurity volume, and confinement duration.[11] The *external heating* systems include three different sources, two of which are high-frequency electromagnetic waves – akin to those

[9] http://www.iter.org/mach/Pages/Blanket.aspx - 03.03.2010
[10] http://www.iter.org/mach/Pages/Divertor.aspx - 05.03.2010
[11] http://www.iter.org/mach/Pages/Diagnostics.aspx - 05.03.2010

working in a microwave oven. These sources operate in concert as auxiliary means of heating in addition to the magnet system. This is necessary as the magnets alone can not heat the plasma sufficiently.[12] Finally, the *cryostat* is a structure that surrounds the core components of the ITER machine hitherto described. It provides insulation for the vacuum and cryogenic temperature required for the superconducting magnet system. Several means of access are integrated in the cryostat design, allowing connection to cooling systems, magnet feeders, external heating, and maintenance for replacement of blanket modules and divertor cassettes. The structure itself is enclosed by a concrete layer called the *bioshield*.[13]

1.4.3. The organization

Today, participants of the ITER project include the People's Republic of China, the European Atomic Energy Community, the Republic of India, Japan, the Republic of Korea, the Russian Federation, and the United States. These countries, usually referred to as Members, will share operation costs, make in-kind contributions of machine components, and provide personnel.[14] Moreover, the Members have all established domestic agencies that act as liaisons between national governments and the central ITER Organization[15]. These agencies are responsible for organising and executing the procurement of the Members' respective in-kind contributions to the ITER project. For this reason, each agency employs its own staff, manages its own budget, and places own contracts with suppliers.[16]

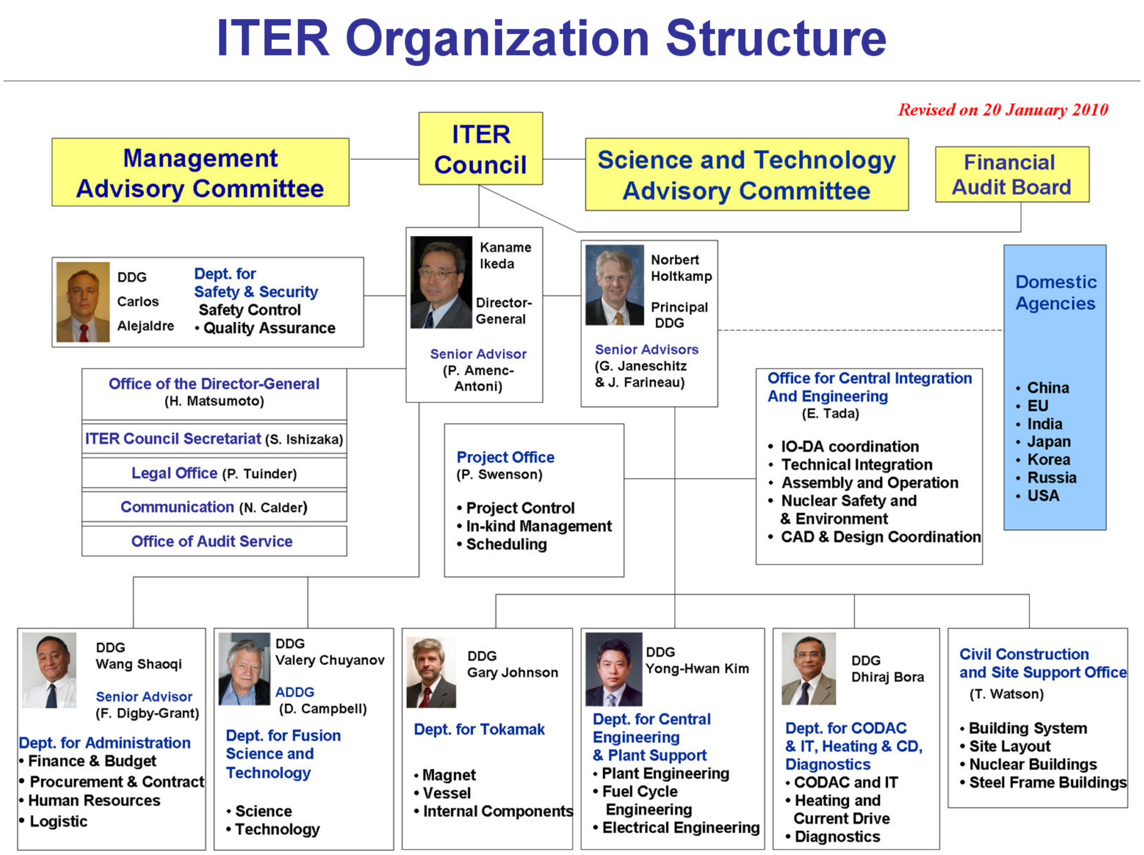
Stationed at the Cadarache site is the ITER Organization and its base. Its departmental structure comprises:

- Administration department
- Civil construction & site office
- Central engineering & plant support department
- CODAC[17] & IT, heating & current drive, diagnostics department
- Central integration & engineering department

[12] http://www.iter.org/mach/Pages/Heating.aspx - 05.03.2010
[13] http://www.iter.org/mach/Pages/Cryostat.aspx - 06.03.2010
[14] http://www.iter.org/org/Pages/Default.aspx . 07.03.2010
[15] In the case of the EU, the European Commission serves as the governmental body.
[16] http://www.iter.org/org/Pages/DAs.aspx - 07.03.2010
[17] Control, Data, Access and Communication: central control system responsible for operating ITER

- Fusion science & technology department
- Office of the director general
- Project office
- Safety & security department
- Tokamak department

Figure 5. ITER organization structure (source: www.iter.org – 07.02.2010)



The work of these departments is supervised by the ITER Council, which is the executive body of the ITER Organization. The council consists of four representatives per participating country, plus two advisory committees.[18][19]

[18] http://www.iter.org/org/team/Pages/default.aspx - 07.03.2010
[19] http://www.iter.org/org/Pages/Council.aspx - 07.03.2010

1.5. Research problem

What are the forces of growth that have made fusion research evolve into ITER, and how may they be accounted for with regards to the particular challenges to which the project has been subjected during the last two decades? It is my claim that there are dynamics intrinsic to particular instances of scientific growth; dynamics which I find significantly comparable to evolutionary innovation processes that are normally being studied concerning the evolution of markets and large technological systems. These dynamics may have implications for energy research policy as well, although they are not necessarily familiar, overt, or even considered significant to the conception, planning, organising, or implementation of science projects. As a consequence, they might exert dramatic influence on science in the long run. Furthermore, I assert that their importance can be demonstrated quite plainly if one regards the contemporary high-stake undertakings of science; those quantitatively unsurpassed in terms of requirements for resources and capital. In this context, ITER is the indisputable representative of the field of fusion research; a position from which it has both profited and suffered. ITER is generally a relevant unit of analysis in view of its connection to the development of possible future energy regimes, and this thesis might raise questions around the premises and strategic bases on which energy-related science projects should be implemented in order to maintain their economic sustainability.

To distinguish, and make use of, the key growth processes leading up to ITER in 1985, I will employ a wide-ranging analytic approach as I am concerned with mapping the fusion field's structural changes that cover over 35 years of development. Therefore, my subjects of interest are not really the different components, actors, or groups involved, but the dynamics they correspond to. According to the disposition of this thesis, I hold that these dynamics will be most usefully construed if treated on a systemic level. Such wideness in scope is necessary for me to be able to explain the patterns of long term processes that I think can be linked to the seemingly unstoppable, and yet self-constraining, momentum of the tokamak technology central to the ITER project. It is my intention to do this by providing suggestions as to how implications of large-scale science in the modern day may be interpreted through perspectives on the concept of Big Science in combination with perspectives deriving from evolutionary economics, collectively known as properties of path dependency. These will be addressed in the following theory chapter.

Chapter 2. Theory

2.1. Big Science

There is little doubt that fusion promises a landmark approach to the future world of power production with its utopian prospect of a limitless and clean energy source. At the same time, the science that needs to be perfected in order to fulfil this prospect is one of overwhelming technological contingency that stipulates a physical, financial, and organisational magnitude of extreme proportions. It is the sense of utopia and prerequisite for scale that has made me think of the discussion related to the term *Big Science.* This discussion is accordingly one that I aim to pursue and draw on in this thesis as I believe it lends an intellectual basis on which my own analyses of fusion and ITER can be made. It should be noted that, in the course of its studies, Big Science has become not so much a definite term as a contextually conditional notion. As I will provide details on below, this notion can be construed roughly as a twofold subject of which one aspect addresses the post-World War II ostensible immediacy of Big Science through large-scale science projects, and another deals with long term perspectives on the phenomenon; the latter being markedly multifaceted and, therefore, pertaining to the rather discursive approach I will be using during this thesis. In addition, the operationalisation of Big Science is to some extent determined by semantics, as put forward by selected authors in the following sections.

Firstly, as a preface to the Big Science discussion and my associated analyses, I find it proper to recapitulate the derivation of the term, i.e. the original conception that was coined and popularised by nuclear physicist Alvin M. Weinberg, and the subsequent extension of its methodological reach by fellow physicist and historian Derek J. de Solla Price.

2.1.1. Background

Weinberg initially used the expression in 1961 in a *Science* commentary entitled "Impact of large-scale science on the United States" (Capshew 1992, p. 5). The article put on view the always majusculed 'Big Science' as a less inelegant substitute for the labelling 'large-scale science', alluding to the vast dimensions of the "new and shining and all-powerful" scientific activities of today (Price 1965, p. 2). Drawing analogies to famous monuments and symbols of ancient times such as the Sphinx, pyramids, cathedrals, and so forth, Weinberg stated that the symbols of our own time are similarly epitomised by "the monuments of Big Science", hereunder sophisticated artefacts which include space vehicles, rockets and missiles, particle accelerators, and high-flux research reactors (Weinberg 1961, p. 2) These inventions, he maintained, are to be considered the most prestigious and important cultural manifestations of the modern world (ibid.). Consequently, the original (Weinbergian) rendering of Big Science evidently portrays the phenomenon as a taken-for-granted paradigm of post-WWII scientific endeavours that are firmly connected to the massive and complex specimens of modern technology. Accordingly, Weinberg's contextualisation suggests that Big Science, in 1961, was synonymous with the roughly 20 lattermost years of scientific accomplishments. Hence, a common implication is that Big Science is indeed a concept of contemporaneity.

It is this idea of contemporaneity and immediacy that would become the focal point of Price's broadening of the Big Science term with his distinguished book "Little Science, Big Science", published circa four years after Weinberg's article. Price sought to deliberate on the presumption among both scientists and laymen that science is essentially a fact of the current, seeing as the major proportion of all scientific activity in the history of man occurs in present time. Alternatively, one can say that 80 to 90 percent of all scientists that have ever lived are alive today, meaning that 80 to 90 percent of the sum of all scientific work will be achieved throughout a normal human lifespan. This postulation was made by Price on the foundation of empirical statistical evidence showing "with impressive consistency and regularity" that any significant segment of science that is measured reasonably will reveal that the normal mode of scientific growth is exponential, in proportion to population growth. I will not delve into the mathematics employed by Price, but simply acknowledge his findings that display a sense of immediacy, as he declares, that works as a powerful characteristic in making science appear primarily modern, and is further supported by the sheer scale and sophistication of the many technologies and methods through which it is channelled (Price 1965, p. 1-2).

In short, Price posits that since the seventeenth century the transition from so-called Little Science to Big Science, if one accounts exclusively for scale as Weinberg does, has followed an exponential growth rate showing a steady doubling of, for instance, published periodicals and manpower approximately every 15 years (ibid., p. 8). The problem is that this brings nothing exceptional to the notion of Big Science that separates it from that of Little Science; in Price's words:

> It follows that [exponential growth of science], true now, must also have been true *at all times in the past*, back to the eighteenth century and perhaps even as far back as the late seventeenth (ibid., p. 14).

> Science has always been modern; it has always been exploding into the population, always on the brink of its expansive revolution (ibid., p. 15).

This is owing to the very function of the law of exponential growth. It is emphasised that in order to analyse peculiarities of Big Science, then, one must seek other phenomena apart from "the steady hand-in-hand climb of all the indices of science through successive orders of magnitude" (ibid., p. 19). Price's proposal to this problem is the possibility of stagnation and ultimately termination of this growth. As he reflects, "in the real world things do not grow and grow until they reach infinity", implying that exponential processes sooner or later arrive at a limit which suppresses further expansion before crossing the threshold of absurdity. Such a function is known as the logistic (sigmoid) curve, which is constrained both by a floor and a ceiling; the former representing zero growth, the latter representing maximum value of growth (ibid., p. 20). The curve's ceiling commands that exponential processes can not continue their regular pattern indefinitely and are forced to incline at some point, typically before the median is reached. Price stresses, however, that growth processes that have remained exponential for a prolonged period of time usually do not easily conform to this change. Instead, they will attempt to alter their "shapes and definitions" to avoid termination. This is explained by the "cybernetic phenomenon of hunting" in that the growth curve commences violent oscillations when facing a dead end (ibid., p. 23-24).

In other words, exponential growth will finally enter a phase of crisis from excessive maturation, and Price defines (or prognosticates) the modern condition of Big Science as a transient stage to which such a crisis is imminent: Big Science constitutes a paradigm shift in the pattern of scientific progress.

2.1.2. Further work

Price introduced with his ideas a completely new and original approach in reviewing the nature of scientific development, and established an intellectual framework from which other authors and observers would further explicate the subject. In the article "Big Science: Price to the Present", authors James H. Capshew and Karen A. Rader examine the concept of Big Science from its point of origin in 1961 and through the resultant discussion in a variety of historical, sociological, and policy studies. As a primary method, they distinguish between 1.) science *being* big – an approach closely related to that of Weinberg's which is concerned with the contemporary scene and how one can observe the consequences of Big Science, and 2.) science *becoming* big(ger) – an evolutionary approach that deals with the transition of 'Little Science' into Big Science, drawing greatly on Price (Capshew 1992, p. 4). Their procedure reads as follows:

> One way of sorting out the issues we have raised is to make an explicit analytical distinction between "Big Science" as a rhetorical construction that has pointed to certain features of contemporary science following World War II and "big science" as a generic label for the forces of growth that have propelled the scientific enterprise since the seventeenth century (ibid., p. 22).

Now, to turn this discussion into one that can be used to help us link the evolution of fusion with ITER's current predicaments, I will first clarify my perception that Capshew and Rader's differentiation between *Big Science* as rhetoric in opposition to *big science* as concrete growth impulses can be framed as *static* versus *dynamic* interpretations. This means that Weinberg's original view and its related themes are static in that they treat size as the dependent variable, primarily seeking to define Big Science in terms of various observable phenomena currently taking place *once they have reached a significant level of size*. Additionally, the only explicit acknowledgement of time in this view is the premise that Big Science is exclusive to the post-WWII era. The alternative view, then, is dynamic in that it is concerned with Big Science as a general moniker for the actual processes that make science develop and mature over shorter and longer periods of time; after and prior to WWII. The dependent variable here is therefore growth or development. I find this static/dynamic distinction to be an intriguing dichotomy that I believe can function well as an analytical tool to illustrate that concepts of size versus concepts of growth in Big Science might have a propensity for divergence that is more than merely theoretical. I also maintain that this divergence corresponds to Price's ideas on growth rate, saturation and crisis, which I will point out in the main analysis.

2.1.3. Static interpretation

Is ITER a Big Science project in the conventional sense? In my view, the static interpretation will confirm this. This distinction of Big Science has been used to explain the nature of a variety of post-WWII science projects, e.g. the Human Genome Project (Westfall 2003, p.

34). It thus corresponds to the literature that builds on the awareness that Big Science is a modern phenomenon and that growth of science has significant implications for modern society. For decades, authors have explicated the various symptoms of growth which, in turn, Capshew and Rader use to organise Big Science into suggestive subcategories:

- Big Science as a pathology
- Big Science as a scientific phenomenon
- Big Science as an instrument
- Big Science as industrial production
- Big Science as an ethical problem
- Big Science as politics
- Big Science as an institution
- Big Science as culture
- Big Science as a form of life

(Capshew 1992, p. 5-18)

With regards to my approach to the ITER analysis, the static notion of Big Science is arguably most applicable in terms of the *instrumental*, *industrial*, and *political* contexts. These I believe represent important propositions as to how the ITER project might be labelled 'Big Science' in conceptual terms and why size has been considered an asset in science, particularly during the 1950s, 1960s, and 1970s – a point that should be stressed especially when looking at traits of technological optimism and the political motives for the ITER establishment in 1985. I will elaborate on this later in the text.

A natural question, then, is whether ITER is an authentic Big Science project in the literal sense of the term. Typically, Big Science is connected to physical *scale*, i.e. "the sense of large masses in one place and the industrial connotations associated with scaling up" (ibid., p. 23). A typical implication of this is the vertical integration of both human and material resources, centralised power and regulation, and the spatial "concentration of work processes within a circumscribed locale" (ibid.). There is also a complementary opinion that activities which size relates to *scope* are Big Science, meaning that they call for coordination among investigators or different facilities that are geographically dispersed, and therefore relying "on extensive communications networks and decentralised work processes" featuring horizontal integration as their hallmark (ibid.). Finally, and perhaps more ambiguous, the *significance* of activities has also been identified as a plausible Big Science indicator. However, I wish to add that this determinant, if used, is secondary in that it is dependent on either the scale or scope of scientific activities as a supplementary variable. As a result, a science project will almost categorically be regarded as 'big' in terms of significance (or impact) once it is "sufficiently broad in scope or large enough in scale" (ibid.).

In the case of ITER, both the requirements of scale and scope are met; an observation which I will now attend to. Below follows a presentation of the selected three subcategories (Big Science as an instrument, Big Science as industrial production, Big Science as politics) from Capshew and Rader's rhetorical Big Science arrangement in order to typify the ITER project as a classic post-WWII Big Science venture.

a. Big Science as an instrument

It is stated that technology has had a close relationship with Big Science from early on as a key feature (arguably the main motivator). Monumental technologies such as accelerators and space vehicles have therefore been highlighted as characteristic of Big Science; projects that have been implemented exclusively through the support and regulation of governments due to their scale and expense. In addition, it has been underlined more generally that technological innovation serves as an impetus for scientific change and that technology neither derives from nor is subordinate to scientific theorising, but that long term patterns prove techniques as equally influential as ideas (ibid., p. 8). Price argues:

> In short, the scientific revolution, as we call it, was largely the improvement and invention and use of a series of instruments of revelation that expanded the reach of science in innumerable directions, and almost fortuitously (ibid.).

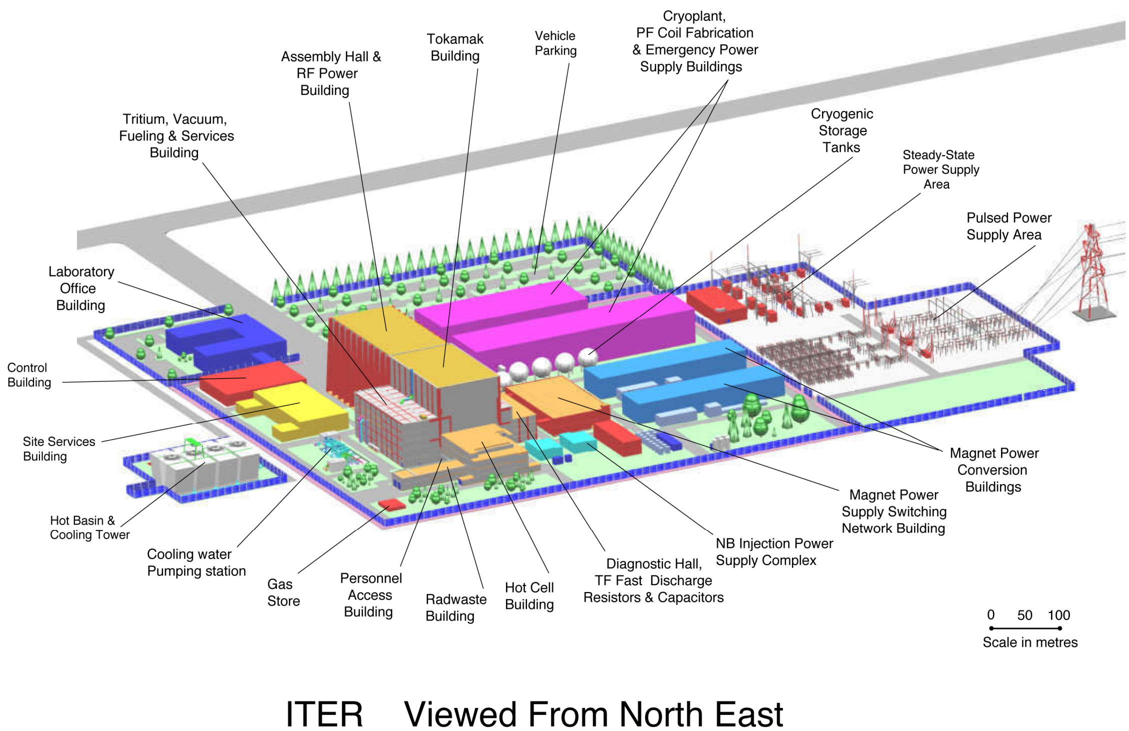
In view of this, instrumental dimensions of Big Science have been studied both in "the literal sense of its technological components" and in the figurative or "metaphorical sense of its use to serve conjoined political and cognitive goals" (ibid.). The latter can, for one, relate to the political leverage that large-scale research projects may provide to its patrons and their organisations. For instance, one can well contend that it is in fusion scientists' interests that fusion technology is made bigger and more complex in order to attract the needed attention and precedence of governments, and the strategies deployed in the pursuit of these interests are largely formed within the milieus of fusion, i.e. laboratories, workshops, conferences, etc. A metaphorical-instrumental idea is also used to address duality in Big Science with the claim that a particular activity is not merely about its most immediate operations, but just as much the symbolic utility of the system that these operations develop or in which they partake. As an example, Capshew and Rader's article brings up the advent of spaceflight in that it is not merely being "a tale of the gumption and luck of Russian, German or American rocketeers", but just as much the notion of command technology as a symbol of the modern state. More generally, it is asserted that state leaders regard science as a source of "instrumentalities" that are not directly connected to political power in the conventional sense, but to a material force that complements political power in the shape of weapons, techniques, communications, etc. (ibid., p. 9).

Although metaphors may be symptomatic also in the case of ITER, it is in the literal-instrumental sense that the project can be most plainly situated. In this concern, Capshew and Rader refer to the technology-centred nature of Big Science and how it has generated further challenges and problems in terms of practical engineering in scientific operations, demanding additional technological equipment and know-how in order to support them. It is held that the more scientific instruments increase in volume, the higher the probability of these instruments converting into technological systems of complex magnitudes, commanding "industrial-scale inputs of capital and labour" (ibid., p. 8). I hold that the proceedings of fusion correspond well to these findings as the core problem of research has always been, and still is, manipulation of plasma by means of machinery. With the foremost areas of manipulation including sufficient heating, compression, and confinement, and the physical nature of plasma behaviour making this manipulation a strenuous task, fusion reactor technology has gradually become larger and more complicated. Furthermore, it has undergone numerous incremental innovations, such as the introduction of heat- and radiation tolerant materials, and superconducting technology to enhance the strength of electromagnets.

ITER is unique given that the site at Cadarache will comprise the highest density of large-scale technological apparatus for a single research project (ITER-FEAT Outline Design Report 2000, p. 33). I will point out a few examples of these to provide a basic conception of the instrumental magnitude and complexity involved.

Figure 6. ITER Cadarache site

(source: http://www.naka.jaea.go.jp/ITER/official-J/pics/ITER_site_2002.jpg)

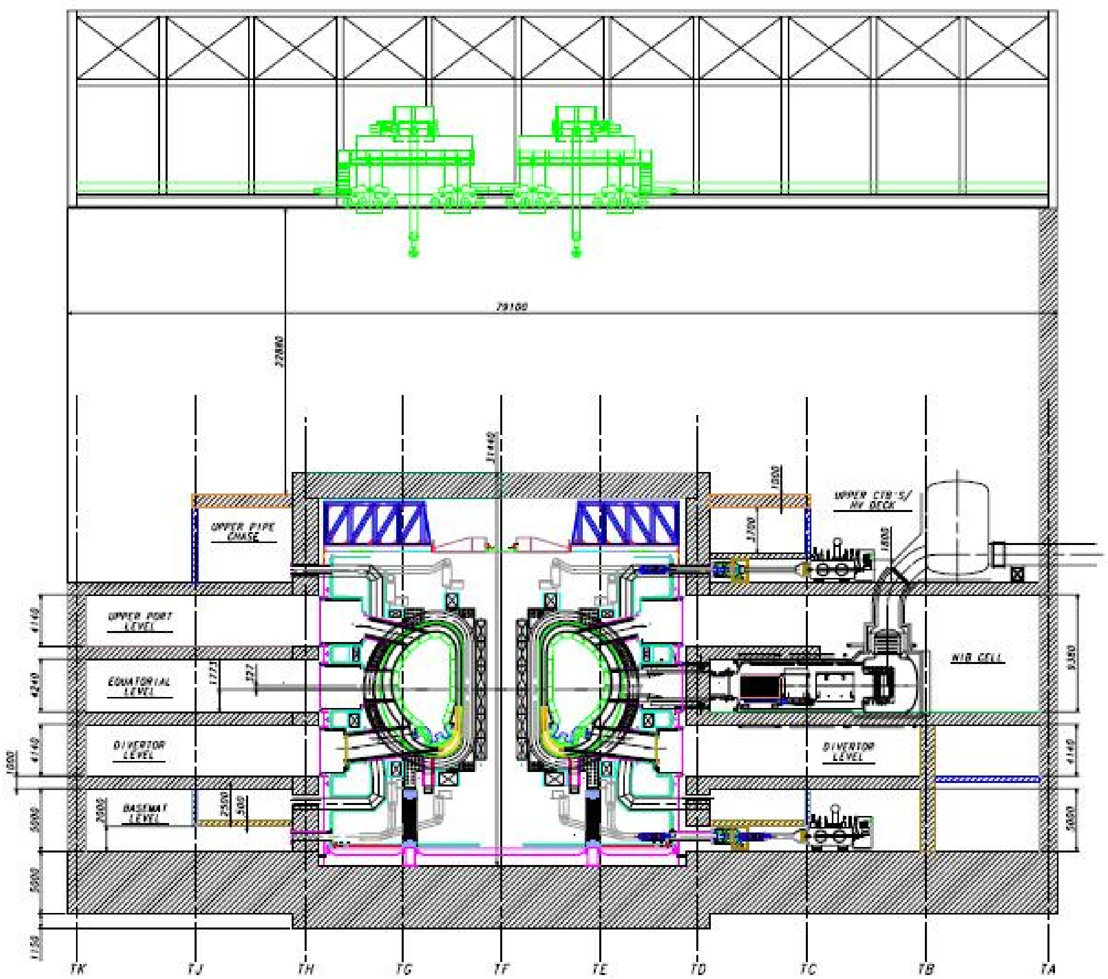


In terms of machine hardware mass, the ITER test reactor alone consists of about 36 000 tonnes of metal and instrumentation, with the magnet system and vacuum vessel being among the heaviest components. The magnets feature 18 TF coils with an individual weight of 360 tonnes (equalling a fully loaded Jumbo jet)[20], 6 PF coils with individual weights ranging from 130 to 385 tonnes[21], while the vacuum vessel consists of 9 sectors at a combined mass of 5 000 tonnes[22]. The dimensions of ITER components are just as vast, including the central solenoid coil measuring over 4 metres in diameter and more than 12 metres high (Schultz 2005, p. 2), the overall machine core measuring approximately 24 metres and 30 metres wide[23], and the surrounding cryostat at 31 metres high and nearly 37 metres wide[24]. The ITER

[20] http://www.iter.org/Pages/FactsFigures.aspx - 13.03.2010
[21] http://eidi.f4e.europa.eu/procurement_package_details.php?id=11 - 13.03.2010
[22] http://www.iter.org/mach/Pages/VacuumVessel.aspx - 09.03.2010
[23] http://ec.europa.eu/research/research-for-europe/energy-iter_en.html - 14.03.2010
[24] http://www.iter.org/mach/Pages/Cryostat.aspx - 09.03.2010

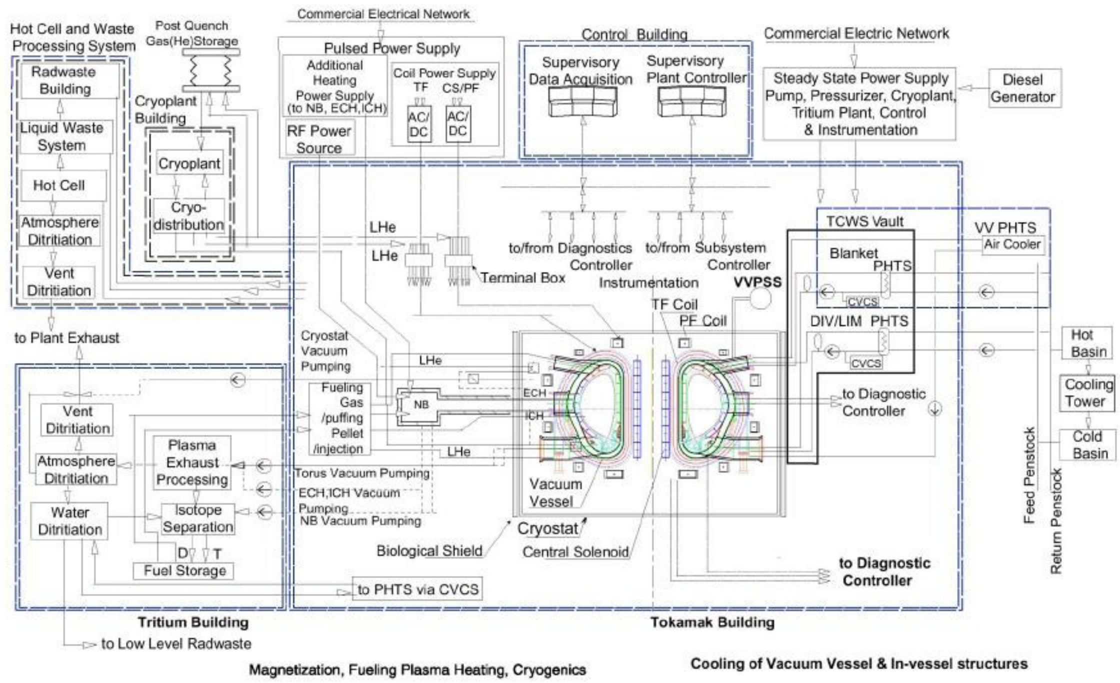
reactor system will be housed inside the concrete bioshield in a specially designed building (see figure 7).

Figure 7. Cross section of ITER tokamak building with subterraneous machine installation (source: ITER-FEAT Outline Design Report 2000, p. 33)



Furthermore, the external systems planned for the ITER site includes robotic remote handling for replacement of blanket modules; vacuum and cooling devices; dedicated power supply in the range of 110 to 620 MW during plasma pulses, and various assembly, storage, testing and maintenance facilities, etc. (ITER-FEAT Outline Design Report 2000). Figure 8 displays a schematic illustration of the technical structure at Cadarache, giving an idea of the intricate network that is constituted by the most immediate instruments required for operation.

Figure 8. Basic plant system configuration (source: Summary of the ITER Final Design Report 2001, p. 32)



Another aspect of this technological maze is the considerable scale of the underlying software systems divided into three hierarchical tiers called CODAC, Interlock Systems, and Safety Systems. CODAC, providing the ITER control, data access and communication functions, is designated for managing command operations and will enable full integration of the entire ITER plant. Its functionality involves continuous monitoring of all systems, preparing and automating of scheduled operations, and data recovery, storing and structuring. The Interlock Systems will safeguard the ITER machine and other parts of the ITER plant to avoid or minimise damage should operations become faulty, while the Safety Systems will protect the environment at the site and the personnel working there. Possible protective actions may be inhibiting access to potentially hazardous areas and termination of plasma sequences (Lister 2006, p. 1).

b. Big Science as industrial production

A consequence of the highly technological orientation of Big Science, in combination with profound investments in working facilities and infrastructure, is the so-called industrialisation of science. This is typically reflected in the 'mass production' of knowledge, treating it as a commodity to fulfil governmental expenditure quotas. However, a more subliminal extension of scientific mass production is the industrial undertone in the corresponding organisational structure. Observations put forward that the natural unit within the organisation of scientific research is the technique, e.g. nuclear reactors, rockets, lasers, computers, etc., around which differentiated groups of personnel perform highly specialised technical functions and operate under various forms of hierarchical management. This has made the analogy to industrial production and administration "inescapable" (Capshew 1992, p. 9). A noticeable example of this understanding is the organization of ITER where critical scientific findings are contingent on a machine which is developed, built, operated and interpreted by a broad workforce that is vertically differentiated and horizontally specialised in terms of functions and geography. Hierarchical proportions can be observed even in the preliminary project design phases that followed the 1985 inception of ITER. These phases were highly scientific and explorative in their own right, seeing as several requirements for the ITER machine, such as adequately heat- and radiation-tolerant materials, were yet to be invented.

At first, a working group, originally consisting of Americans and Soviets, was formed in 1987 to establish technical goals and objectives for the ITER project. At the insistence of the United States, the Japanese and European associates were invited to join as full partners in ITER, with the European nations acting as a single group through the EU in Brussels. The project was to be organised under the auspices of the International Atomic Energy Agency (IAEA), and all of the meetings of the working group were held at IAEA headquarters in Vienna. This led to the first design phase called the Conceptual Design Activities (CDA), initiated in 1988 and scheduled to be finalised by December 1990. The bulk of the work was to be conducted at the German fusion centre at Garching by a team consisting of roughly 50 scientists and engineers divided equally among the four partners. The European site location granted priority to the United States, the Soviet Union and Japan when leadership roles were assigned (Fowler 1997, p. 118). The second ITER design phase was called the Engineering Design Activities (EDA). It was a six-year programme, signed in 1992 by the four parties, with the objective of delivering detailed machine specifications that would provide a decision basis for moving on to the actual ITER construction phase. During the EDA, industrial connotations were augmented through the commissioning of machine component test models, accompanied by a further accentuated hierarchy and task specialisation that featured a dedicated 'Joint Central Team' that included around 170 scientists and engineers working full time for ITER. In addition, the Central Team was supported by several specialists working part time in the so-called 'Home Teams'. What is more, managerial issues demanded greater priority. Due to disagreements regarding a single site for the Joint Central Team base, a compromise decision was made to split the team between three international sites in Garching, Naka and San Diego. The Garching site, running under American management from Massachusetts Institute of Technology (MIT), was appointed the responsibility for the vacuum vessel and the hardware it contained. The Naka site, lead by a European supervision group from the world's currently largest tokamak installation, the Joint European Torus (JET), was to be responsible for the hardware outside the vacuum vessel, including the magnets, buildings, and power supplies. Finally, the site in San Diego, working under Russian direction, got the responsibility for overall coordination, physics, and safety. San Diego would also be the headquarters of the ITER director (Braams 2002, p. 251). The Russian Federation, which replaced the Soviet Union as an ITER partner, proved unable to host a site of their own as a result of economic problems. Yet, they remained faithful to their commitments to provide resources to operate at the other sites. This global organisational formation of the project, enabled it to function continuously (it was said that "the sun never sets on ITER") relying on widespread interconnections through electronic communications, exemplifying the relevance of scope to Big Science, as defined by Capshew and Rader. The EDA included an immense amount of technology development work carried out by each of the partners on a contractual basis with the ITER director. This meant that the different tasks would be mutually agreed upon and credited proportionally to the respective partner's financial contribution to the project (Fowler 1997, p. 123). Today, ITER is organised much in the same way: the central ITER Organization at Cadarache works as the new Joint Central Team; the ITER domestic agencies resemble the Home Teams; and the procurement of each partner's in-kind contributions to the ITER machine resembles the delegation of contributions to the divisions of the Joint Central Team.

c. Big Science as politics

A common viewpoint is that Big Science is essentially political with its major accretion of central resources predetermining the use of power in one way or the other; a power mostly exerted from the authority of national governments. The United States, for instance, holds a history of science being procured in service to the state for a number of reasons, and the extent and complexity of Big Science projects have made the involvement of institutional, bureaucratic, and national politics inevitable (Capshew 1992, p. 12-13). World War II is a typical case in this perspective as it is known to have "forcefully underscored the links between science and politics", begetting a new era in the interaction between the American scientific community and the federal government (ibid., p. 13). Hence, scientists, primarily physicists, were granted both influence and status on account of their seemingly imperative efforts to aid their country in winning the war, and this position offered them significant benefits which were keenly pursued after obtaining exceptional support for "basic as well as applied research" (ibid.). With the escalation of support and the resultant increase of scientific activity, policy issues became a natural centre of attention, often dealing with the question whether the scientist's role should be on top levels in the political arena or limited to the contribution of technical expertise (ibid.).

As I will address more closely later in this text, the research into fusion, which has culminated with the ITER project, was politically stimulated in its early stages both as a parallel and compatible effort to the work on thermonuclear weapons and later as a separate venture aiming towards potential large-scale energy for peaceful uses. During this period, the organisational aspect of fusion research turned ever more many-sided; from the egalitarian structure of small and independent laboratories to bureaucratic institutes where the stakes and interests of governments and the civic society played a much greater role. ITER has turned into a multi-billion dollar science endeavour which involves and affects not only the principal personnel in the ITER Organization, but also its participating nations at large in addition to representatives from material industry and other businesses that have become committed to carry out the project. Another political feature relevant to this fact is the allocation of funds which, in a democratic society, calls for prioritisation among competing interests. Capshew and Rader highlight that the decision-making in this situation is often more dependent on "judgments of relative value than on scientific knowledge or technical feasibility" (ibid.). This was stated in a *Science* editorial in 1956:

> To launch a satellite requires some knowledge about the laws of physics, but the decision to use that knowledge is not itself a matter of physics. The decision rests on a complex system of values which….culminates in the judgment that available funds should be spent to [further scientific progress] rather than….to reduce the national debt (ibid.).

2.2.1. Dynamic interpretation

In opposition to the majusculed 'Big Science' term, Capshew and Rader's minusculed 'big science' term is based on a critical response to the Weinbergian typology in that it seeks to recognise large-scale research efforts prior to the second half of the twentieth century (clearly influenced by Price), and that the preoccupation with size should be restrained in order to usefully employ the 'evolutionary approach' of studying how science matures over time. This relates to a broader attempt to formulate a new interpretive orientation for the analysis of large-scale research, as is evident in the lesser focus on "the attributes of individual end results" in favour of increased attention to "process and growth", denoting a drift towards treating large-scale research in a less atomistic point of view in place of a more holistic one (Westfall 2003, p. 34).

Suitably, the dynamic interpretation of Big Science is introduced by first pointing to the creation of *dramas of scale* in which the static intuitions about Big Science, as well as scientific understanding and understanding of science in general, is entrenched. This is evident in analyses of the nature of science itself where smaller and larger contexts, in addition to shorter and longer periods of time, are juxtaposed by various authors and others (Capshew 1992, p. 18). The instant outcome of this methodology is the definition of Big Science as the counterpart to something smaller, usually referred to as Little Science – an a priori principle of the static interpretation. Then again, the growth of science is not given; something needs to cause this growth, and it is this 'something' that has the interest of different authors of the dynamic school of thought. Capshew and Rader are among these. They suggest that certain pioneering research activities contribute more to progress and growth than others, and that these have all operated on a high capacity level at some point; that they, in themselves, *are* Big Science by having constituted salients of development and therefore worked as concrete growth impulses. This connotes a temporally focused process wherein the *absolute* size of science may indeed be increasing over time, but also that the *relative* size of scale, width of scope, and degree of significance indicate an understanding of Big Science as a dynamic continuum and not merely a discrete marker of recent outcomes.

Hence, quantitative size need not be a deciding factor, nor should "the litany of money, manpower, machines, media, and the military" that serves as "a convenient mnemonic for the chief features of Big Science" be perceived more than simply a contemporary part of the Big Science puzzle (ibid., p. 4).

a. Network factors of growth

I believe there is merit in the abovementioned musings, but I also think one should dig deeper to steer clear of the methodological black-boxing that may follow the term 'research activity' or 'scientific acitivity'. How can one define a research activity as a force of growth? More precisely; what network of mechanisms enable a research activity, such as fusion research, to involve ever more people, ever larger and more expensive machines, and ever more politics with the result of actually raising the global level of science? In my point of view, it is this question that recapitulates the dynamic Big Science interpretation; I argue that Big Science as a designation for Capshew and Rader's distinction on the forces of growth may benefit from more eclectic and network-based discussions by its proponents, in accordance with the 'global perspective trend'.

What, then, may characterise network factors that constitute growth in science? In the article "Rethinking Big Science: Modest, Mezzo, Grand Science and the Development of the Bevelac, 1971-1993" by Catherine Westfall, a reference to Robert W. Smith displays an early case, more specifically in 1989, of evolutionary thinking. Smith describes the development of the Hubble Space Telescope "in terms of the interplay of technical and nontechnical forces" and by focusing on "the utility of viewing large-scale research as a series of systems and networks rather than stand-alone projects" (Westfall 2003, p. 34-35) This study was later on complemented by Smith and Joseph Tatarewicz by investigating "the development of the space telescope's sophisticated camera in terms of a 'heterogeneous network' that included technologies, institutions, and social networks" (ibid., p. 35). In the same way, network theory has been utilised in the studies of "the institutions surrounding large instruments" such as "the network of multi-institutional collaborations in high-energy physics", "the *system* of national laboratories", and single laboratories which history is revealed as intertwined in "different networks of events, institutions, instruments, politics and people" (ibid.). Linked to this notion is the German definition of "Big Science" ("*Großforschung*") that underlines "the scientific, technical, social, and political connections necessary for large-scale enterprises" (ibid.).

To understand how the evolution of fusion has been allowed to reach the stage in which it currently remains with ITER, I hold that the network growth factors can in some measure be attributed to the three main variables I have identified in the history synopsis which I will pay attention to in the next chapter. These variables are the *political framework* surrounding fusion research, the *scientific community* of fusion researchers and their work, organisations and countries, and the development of fusion reactor *technology*; all notable impulsions of progress within the field of nuclear physics as well as nuclear engineering. My attribution is, of course, limited to the growth of fusion and should thus not be categorically construed as representative to other Big Science projects or scientific enterprises in general. Nevertheless, the network approach is expected to work as a valuable perspective on *what contributes to growth* in fusion, and perhaps more importantly, what contributes to *the need for continued growth* in fusion. Finally, it is my intent that this modus operandi will provide a tangible framing of the dynamic interpretation of Big Science.

2.3. Concepts of path dependency

I contend further that a natural extension of the discussion on Big Science and, hereunder, network growth factors, is the theoretical congruence found in ideas and concepts from evolutionary economics, more specifically those concerned with path dependency. A number of evolutionary economists look at path dependency as a basic approach in their field, possibly owing to that similar ideas are found more than a century back featuring, among others, Carl Menger's 1883 analysis of 'institutional emergence' and Thorstein Veblen's work in 1898 on 'cumulative causation' in investigating the evolution of habits and conventions. However, the rise in popularity gained by the idea of path dependency during the 1980s is chiefly ascribed to Paul David and his work on the economic history of technology, and Brian Arthur's study of non-linear economic processes. Subsequently, the scope of path dependency and associated approaches has expanded well beyond the limits of economics with the introduction to fields such as anthropology, history, political science, sociology, and management studies (Martin 2006, p. 398).

2.3.1. Path dependency

The term *path dependency* relates to path-dependent processes and systems. These are defined first and foremost by the mechanism of 'non-ergodicity' or the "inability to shake free of their history". Put differently, the evolution of a path-dependent process or system is a consequence of its own past (ibid., p. 399) and hinges on how 'adoptations' or 'adaptations' accumulate (Bergh 2005, p. 7). Non-ergodicity is found in a number of contexts, including developmental sequences (e.g. in evolutionary biology and physics) in addition to social dynamics (meaning "social interactions among economic and political agents that are characterised by positive feedback") (Martin 2006, p. 399). In economics, path dependency signifies principally three interconnected varieties:

- path dependency as technological lock-in (as advanced in the works of Paul David)
- path dependency as dynamic increasing returns (as advanced in the works of Brian Arthur)
- path dependency as institutional hysteresis[25] (as advanced in the work of Douglas North and Mark Setterfield)

(ibid.)

Technological lock-in refers to the phenomenon that certain technological fields are inclined to becoming fixed within a specific (and often mediocre) trajectory, notwithstanding the co-existence of alternative, and possibly better, technologies and solutions. This is because of the technological fields themselves being results of events that have occurred in the past. The term *dynamic increasing returns* holds that development paths are reinforced by a process of increasing returns, i.e. positive feedback effects that are generated by a range of externalities. *Institutional hysteresis* indicates that "formal and informal institutions, social arrangements and cultural forms" tend to reproduce themselves over time, and that this transpires to some extent through "the very systems of socio-economic action they engender and serve to support and stabilise" (ibid., p. 400). As I have originally mentioned, the methodological purpose of applying path dependency on the historical circumstances concerning fusion and ITER rests on the content of the network growth factors addressed in the Big Science discussion. And

[25] May be translated as 'inertia', i.e. resistance towards change; e.g. organisational inertia.

with the nature of fusion and ITER history being highly technological as well as politically and socially embedded, I consider David's mechanism of technological lock-in, which joins all three characteristics, to be the most appropriate reference point in this thesis. I should stress, however, that David's features will be used in a rather eclectic manner as I am not attempting to give an isolated explanation of the technological progress in fusion, but rather a comprehensive proposal as to how fusion research has evolved into a fairly rigid pattern with the overwhelming tokamak focus, and has seemed to reach a level of *negative research lock-in* with ITER.

a. Technological lock-in

In his work on path dependency, David presents different features, three of which have been given special attention by other observers and authors. The first one is general in evolutionary thinking and deals with the effect of coincidence. David states that marginal, "historically contingent 'accidents' or microlevel 'chance events'" form a sequence of changes that, in total, might pose long term outcomes that shape the evolution of economic technologies, organisations and systems (ibid.). This can give rise to a volatile process that is especially sensitive to initial events (Bergh 2005, p. 7). Hence, the accidental characteristic of path-dependent dynamics typically transcends any methodical measure of regulation (Martin 2006, p. 401). Related to this, David presents a second general notion postulating that given certain conditions, early actions may leave an imprint on the history that follows them by "closing alternative paths and validating a particular path" (ibid.). Non-ergodicity entails a chaotic factor, meaning that there is an absence of regularity and repetition in temporal data. As a result, the internal diversity of path-dependent processes and systems limits the prospect of these returning to earlier states, whereas each state, in turn, limits the potential of future paths (Bergh 2005, p. 7). This implies that ensuing conditions are prone to non-rationality and sub-optimality, meaning that path-dependent technologies, organisations, and systems likely will evolve into comparably poorer configurations, as cited above (Martin 2006, p. 401). Thirdly, David displays in several writings a more specified focus with the discussion on technological lock-in per se. This phenomenon is a function of another three factors that are occasionally linked to the workings of 'positive network externalities':

- technical interrelatedness

- economies of scale
- quasi-irreversibility of investments

(ibid.)

Technical interrelatedness covers the reinforcing effects of compatible and complementary relationships between diverse technological constituents (ibid.). Compatibility here can refer to matching or congruent constituents that, combined, make up a functional system of variable success. Complementarity, then, may be translated as highly successful compatibility in that a system becomes greater than the sum of its individual parts, meaning that each of the system's constituents gain an added value separately when operating collectively. The term *economies of scale* comprises the rising advantages of a technology that is being increasingly employed (ibid.). These might be cost benefits obtained by a power plant due to expansion: as capital investments (e.g. machinery) are distributed over an increasing number of output units (e.g. cities), the marginal cost of machinery operation decreases. Lastly, David points up the *quasi-irreversibility of investments*, referring to the inertia of sunk costs. Sunk costs, or retrospective costs, are expenditures that have been previously incurred and, for various reasons, can not be recuperated. David proposes sunk costs inertia as a potential cause of technological lock-in as it is thought to restrict the ability of switching "technology-specific capital and human skills to alternative uses" (ibid.).

Coincidence, closing and validating of future paths, and positive network externalities are all features of path dependency that can provide an accompanying perspective to the dynamic interpretation of Big Science as network growth factors. In fact, they may even provide supplementary utility to this level of the Big Science discussion as they put forward premises on which network growth factors are both established and maintained.

b. Momentum

As a supplement to David's ideas, I will borrow the term *momentum* as described by Thomas P. Hughes in his work "The Evolution of Large Technological Systems". Hughes declares that technological systems that have a long-standing history of maturation probably will acquire momentum at some point. In brief, momentum can be explained as a system's accumulated mass which is characteristically constituted by "technical and organisational components" that operate towards a particular objective with a given growth rate "suggesting velocity" (Hughes 1993, p. 76). Hence, significantly mature systems are equivalent to the *inertia of motion* in that they become difficult to stall; a quality that may give the impression of autonomy (self-sufficiency) (ibid.). Furthermore, Hughes pays attention to the technological *trajectory* that is suggested by continuing stability of a technological system's artefacts and knowledge base. This is closely related to momentum (ibid., p. 77). Finally, it is important to emphasise that the notion of momentum does not implicate autonomy, meaning that a highly mature system can be slowed down, changed, and even discontinued. However, either will typically require a considerable amount of time and effort; an essential consequence of motion inertia (ibid., p. 80).

2.3.2 Congruence with Big Science

In order to illustrate the congruence of Big Science with path dependency, hereunder lock-in and growth momentum, one should look for the specific driving forces that make a field of science become larger, possibly turning it into an inherent agent of growth. A basic strategy in the case of ITER and fusion research is to single out reasons for certain scientific methods becoming sooner and more developed and pioneering than others. Interestingly, some of these methods may even include comparably inferior theory, techniques, and technologies, and still retain a somewhat counter-intuitive leading position for prolonged periods of time. Of course, inferiority and superiority are relative terms that encompass a wide array of parameters. For instance, in a highly technology-oriented scientific field such as fusion, parameters may be technological performance, feasibility, sustainability, cost efficiency, time efficiency, political and public legitimacy, etc. By the same token, inferiority and superiority are relative to time, e.g. the differential between developmental stages. This is observable in that, for instance, a pair of competing fusion concepts might fluctuate in relation to dominance, usually until one prevails when reaching the threshold of momentum; a temporally synchronous perception. A temporally *asynchronous* perception is used when comparing a matured, established concept with an emerging, less established one. Hence, the former perception is helpful when studying the real time occurrence of path dependency mechanisms, whereas the latter is particularly suitable when examining how path dependency can lead to a state of lock-in in the sense of older and bigger concepts 'locking out' newer and smaller ones.

Chapter 3. History analysis

3.1. ITER background

The ITER Agreement, which had its origins in diffusing international conflict of the second half of the 20th Century, will likely be an important milestone for peace and prosperity in the 21st Century and beyond (Orbach 2006)[26].

This statement, and others alike, reflect aspects of the general opinion on ITER's background; a somewhat inconclusive version. Although an evident implication of ITER is the unifying effect of joint scientific and industrial labour between nations, the perception of the project's main function as a political Big Science effort to diffuse Cold War tensions appears overly rational and simplistic in its scope. It tends to marginalise the variety of forces and interests behind the project that resulted in the particular framework that has remained ever since. Indeed, it seems reasonable to deliberate on whether the selection of fusion research as the suitable catalyst supposedly for better integrating the Soviet Union into the world economy was rather one of bounded rationality and political satisficing. For instance, state leaders were not entirely free to choose among scientific fields or technologies for cooperation. What were the reasons for their settling on fusion, hereunder the tokamak approach?

The history of ITER can be traced back to the beginning of fusion research in the early 1950s. The tokamak reactor concept on which ITER has been centred since 1987, assumed the leading position in 1969/1970 after 20 years of both competing and collaborative scientific activities worldwide, research declassification, and experience from strenuous problems and solutions. At the 1985 ITER inception, then, toroidal magnetic fusion and the tokamak device had stayed the primary focus of research within its community for approximately fifteen years following the 'tokamak revolution'. Compared to other related technologies and approaches, it had accumulated the community's most extensive knowledge base, networks, infrastructure, and expenditures. Also, the history of fusion is characterised by both formal and informal collaboration between, amongst others, Soviet and American scientists, providing a broad foundation for a true international endeavour comprising the Cold War protagonists. Certain

26 http://www.er.doe.gov/News_Information/News_Room/2006/ITER/ITER%20Initialing%20Ceremony%20Press%20Remarks.htm – 27.01.2010

scientists also had strong political influence and played an important role in promoting fusion as an eligible candidate for formalised international coordination, and the final establishment of ITER. Hence, state leaders were neither the project's sole architects.

Furthermore, during the 1970s, fusion research had become an area of science which stirred both public and political consciousness in terms of oil shortage, future energy demands and environmental issues. Fusion, an inherently emission-free method of producing energy, served as a promising solution to the growing concerns for the securing of abundant power supplies, and the need to exchange polluting large-scale technologies with cleaner ones. This created a substantial incentive for promoting fusion research further. However, the scientific progress of toroidal magnetic confinement, hereunder the technological development of the tokamak device which had culminated in numerous units worldwide (making up a workable base for joint research efforts), would reach the point where insufficient national capacity became an ultimate obstacle. Fusion research had always been conducted under governmental auspices as its commercial potential was, and still is, uncertain. At the end of the 1970s and into the early 1980s, the prospected costs and specifications for next generation tokamaks – machines that would test the feasibility of an actual power generating fusion reactor – were simply too far-reaching for any single nation to maintain on its own. As President Reagan put it:

> It is becoming increasingly important that we all reach beyond our borders to form partnerships in research enterprises. There are areas of science, such as high energy physics and fusion research, where the cost of the next generation of facilities will be so high that international collaboration among….nations may become a necessity. We welcome opportunities to explore with other nations.... (Orbach 2006)[27]

Hence, I hold that there are three main variables affecting the formation of ITER:

1. The scientific community of key actors determined to funnel their work into a durable format
2. Cold War, environmental issues, and concerns about the escalating demands for future energy shaping the political framework

27 http://www.er.doe.gov/News_Information/News_Room/2006/ITER/ITER%20Initialing%20Ceremony%20Press%20Remarks.htm – 27.01.2010

3. Fusion technology, present in a worldwide system of tokamak machines, maturing and pushing towards the next stage of growth, i.e. through international coordination

The historical progress thus requires a synopsis in order to be coherently applied on the research problem of this thesis.

Accordingly, I will present highlights largely based on the accounts of people involved in fusion research first-hand, or authors having conversed with these and/or observed their work. These highlights are grouped as the three key variables above. I will make a rundown of each variable within consecutive phases that are delineated by respective milestone events critical to the evolution of fusion. Each phase will work as a junction where David's features on path dependency are merged with empirical findings. In the following main analysis chapter, I will evaluate more closely how the dynamic interpretation of Big Science can be operationalised on the basis of these mergers, and thus provide suggestions as to how *Big Science* and *big science* might conflict. Note that some variables will be given more attention than others[28], and that overall emphasis will be put on the United States and the Soviet Union.

3.2. Phase 1: 1951-1958 – beginning and early growth

3.2.1. Political framework

Prior to, and during, the embryonic stage of research into fusion, the prominent political circumstances were those imposed by the Cold War. In a certain perspective, the principal incentive for the Western and Eastern blocs to commence their fusion programmes may be ascribed to the uncertainty and bipolar tension at the time. This is plausible seeing as the proximate cause and formal advent of fusion research in 1951 is credited to the announcement by President Juan Perón of Argentina, asserting that a major advance in fusion technology had been accomplished in his country. Perón was supposedly referring to experiments performed by German physicist Ronald Richter in a specially tailored secret laboratory set up by Perón on the Hewmall island (Bromberg 1982, p. 13); experiments succeeding in the "controlled release of atomic energy at a superhigh temperature of millions of degrees without using

[28] Also, in places where the variables *political framework* and *scientific community* converge significantly, one of them will be omitted.

uranium fuel" (Shafranov 2001, p. 5). Perón proclaimed that the knowledge behind this achievement had been derived from Argentina's own survey of global scientific efforts in designing uranium fission reactors. However, no significant details of the process were released by either Perón or Richter (Bromberg 1982, p. 13). Although the scientific merits of Richter (a physicist of mediocre skills by some accounts) were indeed later refuted, Perón's publication managed to attract the attention of both United States scientists and government officials to seriously consider the possibilities of fusion research (Braams 2002, p. 18). In the Soviet Union, prominent physicists, having learned about the Argentine event, immediately summoned a panel to resolve organisational concerns on the subject (Shafranov 2001, p. 5).

3.2.2. Scientific community

a. The United States

In the context of the arms race, looking at the Americans' successful proceedings in designing thermonuclear bombs maintained their confidence that constructing a working thermonuclear fusion reactor would prove a similarly solvable task. This was an all too optimistic fallacy, of course, as ITER is yet to fulfil reactor-grade parameters at some point after 2016. The idea of generating electricity from the abundant and ecologically clean source of fusion energy has remained the core incitement for scientific communities and their nations to engage the fusion venture up to modern times, but this process has been, and still is, inclined to a number of complications (ibid.). Fusion pioneers of the 1950s, for instance, perceived the *confinement* of 100-million-degree plasmas as the single most challenging obstacle to the release of fusion energy, and thus initiated a skilful and, in due course, satisfactory attack on it. Nevertheless, they did not acknowledge the possibility that scientific accomplishments may not necessarily yield any direct consequences (Furth 1990, p. 5); the complexity of fusion confirms this. In the United States, more specifically the Los Alamos National Laboratory in New Mexico, basic fusion-related physics was recognised already before the end of World War II, but no decisive action was taken to capitalise on its potential until the Atomic Energy Commission (AEC) launched an ambitious classified research programme in 1952 and 1953. This classification was initially due to the concurrent progress in thermonuclear weapons, but one that remained even after the research orientation had later switched from support of military programmes to peaceful uses of atomic energy (Shafranov 2001, p. 2). The research programme introduced three preliminary approaches to fusion technology, carried out in geographically dispersed science centres:

- the closed-system (magnetic force lines closed on themselves) *stellarator*- and *fast linear pinch* concepts
- the open-system (magnetic force lines extended outwards) *magnetic mirror* concept

(Braams 2002, p. 53)

The stellarator concept was conceived by theoretician and former astrophysicist Lyman Spitzer at the Princeton Plasma Physics Laboratory in New Jersey, a laboratory founded by Spitzer to pursue his idea (Fowler 1997, p. 23); the work on fast linear pinch devices was begun at Los Alamos by British physicist J. L. Tuck; magnetic mirrors were put forward by physicist Richard Post at the Lawrence Livermore National Laboratory, California, after basic experiments at the Radiation Laboratory of the University of California, Berkeley (Braams 2002, p. 18).

b. The Soviet Union

As for the Soviet Union, the structure of the fusion research community would set off in a more centralised format. The government was first familiarised with the notion of fusion research in 1949/1950 through a letter written by Oleg Lavrientev, a soldier in the far-east Red Army. It presented suggestions regarding the construction of the hydrogen bomb and, more interestingly, the electrostatic confinement of deuterium nuclei for industrial-scale generation of energy. The letter was sent to the Central Committee of the Communist Party of the Soviet Union, and then diverted to physicist and mathematician Igor Tamm. Tamm, accompanied by his colleague in the Soviet nuclear weapons programme, nuclear physicist Andrei Sakharov, concluded that Lavrientev's ideas were rather naïve, but nevertheless pertaining to "a very important and not necessarily hopeless problem" (Shafranov 2001, p. 3). Accordingly, the Soviet physicists were spurred to develop their own proposals to the problem. By October 1950, Sakharov and Tamm completed the first evaluations of a magnetic thermonuclear reactor (Braams 2002, p. 19). These are momentous in the sense that they predicted much of the basic physics of what would soon develop into tokamaks (Furth 1990, p. 3). In May 1951, following Perón's abovementioned proclamation, a controlled fusion programme was formally launched by the Soviet Government and a Council on toroidal magnetic thermonuclear reactors (MTR) headed by nuclear phycisist and director of the Institute of Atomic Energy (IAE – later named the Kurchatov Institute) in Moscow, Igor Kurchatov, was established. Phycisist Lev Artsimovich was appointed supervisor of the experimental programme in the IAE, while teacher Mikhail Leontovich was responsible for theoretical studies. Moreover, Kurchatov incorporated other institutes in Moscow, Kharkov, Leningrad, Sukhumi, and later Novosibirsk into the work on plasma physics and controlled fusion. The early experimental activities in the IAE were organised in three groups that were primarily concerned with inductive discharges in strong toroidal fields (the toroidal pinch, later known as tokamak) as proposed by Sakharov and Tamm – everything classified, like in the United States and also the United Kingdom (Braams 2002, p. 19).

In 1955, the first tokamak-like machine (TMP) was built. Alas, scientific performance eventually tended to stagnate as temperatures could not be brought to a higher level than 30 electron volts (eV), a meagre yield compared to the Lawson criterion. This was typical for several years to come; there was no real progress either in linear or toroidal pinch systems for a long time beyond this point. The search for other means of plasma confinement was therefore intensified, resulting in nuclear physicist Gersh I. Budker's idea of a direct axisymmetric magnetic system with enhanced magnetic fields at its ends; a relatively simple option that translated into a large mirror installation called OGRA. Smaller mirror models were also studied in addition to a number of very novel proposals, none of which were doing well. This led to a period of more than five years of profound and widespread pessimism towards the feasibility of solving the fusion problem (Shafranov 2001, p. 6).

c. Internationalisation of fusion and research declassification

The first United Nations (UN) Conference on the Peaceful Uses of Atomic Energy, *Atoms for Peace*, took place in Geneva in 1955 upon the initiative of US President Eisenhower. During the occasion, fusion was mentioned solely in the introductory speech by the conference president Homi Bhabha:

> It is well known that atomic energy can be obtained by fusion processes as in the H-bomb and there is no basic scientific knowledge in our possession today to show that it is impossible for us to obtain this energy from the fusion process in a controlled manner. The technical problems are formidable, but one should remember that it is not yet fifteen years since atomic energy was released in an atomic pile for the first time by Fermi[29]. I venture to predict that a method will be found for liberating fusion energy in a controlled manner within the next two decades. When that happens, the energy problem of the world will truly have been solved forever for the fuel will be as plentiful as the heavy hydrogen in the oceans (Braams 2002, p. 20).

Not surprisingly, Bhabha's words stimulated laboratories worldwide to pursue the subject, as evident in the numerous contributions to the 1957 Venice, 1958 Geneva and 1959 Uppsala UN Conferences report work which originated around 1956. For instance, in France there was resurgence in national fusion research after abandonment a few years earlier, and in Japan, the Universities of Tokyo, Nagoya, Osaka and Kyoto commenced pioneering research (ibid., p. 20-21). Previous to the occurrence of these events, though, the complications faced by Soviet researchers as well as the American and British in the attempts of further fusion advancement, prompted Kurchatov to take unilateral action in terms of declassifying the fusion programme while the research that was being carried out in the United States and the United Kingdom was still kept secret (Fowler 1997, p. 23). The Soviet knowledge base accumulated thus far needed to be exposed (hopefully) in order to expand further by learning from the experience of foreign peers and others. Firstly, Kurchatov organised an All-Union conference in 1955 to discuss the fusion work at the IAE, which I will refer to as the Kurchatov Institute hereafter. The research results exhibited at the conference made an important impact and harnessed considerable interest. Secondly, he travelled to the United Kingdom as a member of a Soviet governmental delegation to give a lecture 'On the feasibility of the thermonuclear reaction in gas discharge' at the atomic research centre at Harwell. This is considered the first true move towards international cooperation in fusion. The visit was followed by a Swedish delegation to the Kurchatov Institute the next year, which in turn made possible a Soviet participation in a Stockholm astrophysics conference only months later. Here, Artsimovich and his colleague gave talks that were implicitly connected to their own research programmes on a variety of pinches. The meeting was attended by stellarator architect Spitzer as well as leaders of the British fusion programme. In later conferences, notably Venice 1957, a large number of papers related to fusion were presented by fusion researchers from the United States, the

[29] Enrico Fermi, Italian physicist known for his work on the development of the first nuclear reactor, and for his contributions to the development of quantum theory, nuclear and particle physics, and statistical mechanics.

United Kingdom, and the Soviet Union. All in all, traits of international cooperation were surfacing (Shafranov 2001, p. 7).

Kurchatov's aim to bring about the unrestrained exchange of knowledge in fusion was fully realised with the arrangement of the 1958 Geneva Conference. This would become a vital event in the history of fusion as the nuclear powers of the world collectively agreed to declassify their research activities. Complementary to this, Artsimovich wrote in his overview of the Soviet effort:

> …there begins to emerge a rough outline of the scientific foundation on which the methods of solving the problem of controlled fusion reactions will probably rest….We do not wish to be pessimistic in appraising the future of our work, yet we must not underestimate the difficulties which will have to be overcome before we master thermonuclear fusion….The solution of the problem of thermonuclear fusion will require a maximum concentration of intellectual effort and the mobilisation of very appreciable material facilities and complex apparatus. This problem seems to have been created especially for the purpose of developing close cooperation between the scientists and engineers of various countries (Braams 2002, p. 54-55).

What is more, the participants would also display portions of their most noteworthy fusion experiments in operational condition on the UN premises (ibid., p. 31). The participants exhibited several methods of plasma confinement, or "a fair of ideas", according to Artsimovich (Shafranov 2001, p. 8). These are listed in the technology section below.

3.2.3. Technology

The first decade of fusion research introduced various technological approaches, among which some have laid the foundation for theory and engineering principles still in wide use today. I will present the distinguished configurations here in some detail to provide a basic insight in the earliest technologies that comprised the most primal stage of the evolutionary process in fusion:

- Stellarators
- Steady-state mirror confinement
- Pulsed mirror confinement and theta pinches
- Fast linear pinches/z-pinches
- Toroidal pinches

The *stellarator* concept presented in 1951 by Spitzer was a toroidal magnetic field system with rotational transform. 'Rotational transform' means that field lines, while revolving in the toroidal direction, rotate around a magnetic axis in a helical pattern. The idea of magnetic surfaces built in toroidal geometry derives from Tamm, but while his early work with Sakharov had developed into combining the toroidal field with a poloidal field (see fig. 2) plasma confinement, Spitzer drafted the stellarator as a figure eight shaped like the number '8'), steady-state field without an induced current, meaning that it manipulated the field with external currents instead of driving a current through the plasma itself, like in the pending tokamak (Braams 2002, p. 43).

The confinement concept of *mirrors* had occurred in at least two places: Moscow and Berkeley. As mentioned, Post began his experimental work on mirrors in 1951/1952 at Berkeley and later transferred it to Livermore. In Moscow, Budker initiated a theoretical study of mirror confinement in the Kurchatov Institute. The geometry of the steady-state mirror machines resembled that of the pinch, i.e. cylindrical (tube-shaped). These could be filled with hot plasma by a variety of means such as RF heating/dielectric heating = heating by micro- or radiowave electromagnetism (similar to a microwave oven), particle radiation = heating by injection of fast-moving subatomic particles, or a plasma gun = an apparatus that ejects plasma in the form of a plasmoid (a coherent structure of plasma, e.g. a ball), without compressing it. The mirror machine consisted of two opposite magnetic mirrors, e.g. parallel coils separated by a small distance. These coils produced a magnetic field that temporarily 'trapped' charged particles and thus confined the plasma discharge which then generated energy (ibid., p. 35). Unlike steady-state mirrors, which were based on a single continuous plasma discharge, the *pulsed mirror* variant centred on several energy releases, i.e. pulses, throughout operation. Here, plasma could be created primarily in the same ways as those in its steady-state counterpart, but in several, shorter cycles during which plasma was compressed and heated by a rapidly rising mirror field, as opposed to the steady-state inert field. The *theta pinch* was a subcategory of the pulsed mirror approach, all pioneered by Post (ibid., p. 40). The most detailed work on pinches, hereunder *fast linear pinches/z-pinches*, during the 1950s was reported by the Kurchatov Institute (ibid., p. 31). These were metal-wall cylinders either pre-filled with gas or connected to an array of heavy-duty wires that injected gas during operation. Plasma discharges within the cylinders were then generated by applying a high voltage pulse across an anode-cathode (positive to negative electrode) gap, also known as 'ohmic heating'. This method confined the plasma which was hence imploded (compressed) by the magnetic field produced by the flowing discharge current. During compression and stagnation, the kinetic energy was converted into thermal energy and radiation, forming a hot and dense core at the centre of the cylinder. Lastly, from 1950 to 1960, the architecture of *toroidal pinches* had evolved from small glass tori ('doughnuts') wound with rudimentary cables to well engineered constructions. Some examples showed a torus insulated with a dense conducting shell that worked both as a stabilising chassis and as the primary magnetic coil winding, but mostly the earlier glass or ceramic tubes were substituted by *sectioned* aluminium tori. The *continuous* metal liner was first introduced in slow discharges, i.e. Soviet toroidal pinches that would ultimately be distinguished as a separate class – the tokamaks. Iron cores, fed by capacitor banks, were frequently utilised to enhance the coupling with the primary toroidal current (ibid., p. 50).

When comparing the countries' different technological approaches it became apparent that a typology consisting of four broad categories of confinement systems could be devised:

- Closed magnetic surfaces with steady-state operation
- Closed magnetic surfaces with pulsed-mode operation
- Open magnetic surfaces with steady-state operation
- Open magnetic surfaces with pulsed-mode operation

(Ibid., p. 53)

Spitzer's stellarator was regarded as the most potent design of the conference programme; 'soaring' as the epitome of a stationary magnetic system for plasma confinement – the ideal for fusion (Shafranov 2001, p. 8). It was the only closed system at the time for which steady-state operation could be visualised, making it one of the most noteworthy candidates for a full fusion reactor development (Braams 2002, p. 124). Its status also proved influential towards the Soviet research, yet to the advantage of the tokamak. By realising the weight of Spitzer's proposal, Kurchatov urged a halt in the construction of the third tokamak version T-3, a model which will be more thoroughly addressed below in this text, to do a careful evaluation of the stellarator direction with the intent of pursuing this in place of the tokamak. Kurchatov Institute lead researcher N. A. Yavlinski then had a selection of his employees comparing the new tokamak blueprints with the stellarator properties. In the end, arguments in favour of the tokamak were given on the basis that the tokamak chamber radius was greater than in a stellarator of equivalent proportions, reducing the negative effect of the chamber walls on the plasma discharge. What is more, the heating available at the time was achieved by electrical current mechanisms only, giving the tokamak the upper hand as this system provided a higher current than that of the stellarator. Hence, the tokamak line stayed the Soviet main priority in spite of the stellarator's attractiveness overseas, and the competition between the two reactor candidates constructively worked to intensify the fusion effort (Shafranov 2001, p. 8).

Conversely, at the point of Geneva 1958 there was no definite scientific reasoning for favouring one approach or category over the other, and nor was this yet of much significance. What truly mattered was not so much the construction of a feasible thermonuclear reactor as the aspiration of providing the most valuable contribution to the physics of magnetic plasma confinement in general; a matter of which opinions were split. Sceptics stressed the questions of whether electrical power could at all be generated using deuterium and tritium as fuels by themselves, and, if yes, whether the featured technology could be made economically viable. On the other hand, these reservations were largely ignored as the general impression was that fusion power would undeniably be available within two decades. The optimists were euphoric about the newly revealed world of yet another sensation conceived within the field of nuclear science (Braams 2002, p. 53-54). A comment on this debate, which remains essentially valid today, was made by theoretical physicist and "father of the hydrogen bomb" Edward Teller:

> I think that [thermonuclear energy production] can be done, but do not believe that in this century it will be a thing of practical importance….It is likely that we shall be dealing with an intricate machine which is inaccessible to human hands because of radiation and on which all control and maintenance must proceed by remote control. The irradiation of materials by neutrons and gamma rays will cause the properties of these materials to change….These and other difficulties are likely to make the released energy so costly that an economic exploitation of controlled thermonuclear reactions may not turn out to be possible before the end of the twentieth century (ibid., p. 54).

In some ways, the Geneva Conference of 1958 clarified that the optimism of the early 1950s was rather unfounded and that the field of plasma physics was too immature for the challenge posed by the fusion reactor. The additional knowledge gained from research declassification seemed to alleviate some existing problems, but also confirmed the intricacy of others, and even identified new ones (ibid., p. 55).

3.2.4. Traits of path dependency

Due to the prevalent tension and international covertness of the 1950s, research into fusion was *compatible* with the then-present political climate. With the advent of the nuclear bomb and knowledge of its successful use on Hiroshima and Nagasaki to end World War II, power politics had shifted towards becoming progressively more centred on science and technology. Obviously, both in the United States and the Soviet Union there was a firm resolve to excel in this respect in order to trounce the opposition in what would become the renowned bipolar arms race, as well as scientific and technological race, of the Cold War. Also, considering the focus on developing advanced technology to strengthen national security, it was likely that the venture of fusion gained priority in the United States at a point marked by an additional hot war with North Korea (Bromberg 1982, p. 32).

Perón's announcement was a trivial yet effective action in illustrating the political blind spots and correlating alertness that sustained the bipolar system: neither of the two blocs possessed reliable information on Argentina's fusion issue, then again, neither intended to ignore it owing to its potentially immediate impact on the global balance of power. This is an example of how the structural uncertainties of politics, when amply acute, can facilitate new technological and scientific ventures when petty actions like that of Perón's are made. Hence, Perón's announcement may be accounted for as a political *microlevel chance event* in that it occurred as a relatively marginal and unexpected incident in the broader Cold War context, but, nonetheless, steered the Americans and Soviets closer to a possible fusion trajectory. Another important characteristic of path dependency at this point in time was the contours of an international fusion scientific community and related knowledge bases emerging in the early 1950s, mainly represented by the United States and the Soviet Union which mobilised their initial movement towards exploring the possibilities of fusion power, and later connected with each other through scientific collaboration and overt competition based on fusion for peaceful uses. Research was introduced by a select number of laboratories in both countries, directed mostly by prominent nuclear physicist under governmental auspices. Fusion was a completely new and unfamiliar field, drawing purely on conventional theoretical physics, but nevertheless motivated by the contemporaneous success in constructing thermonuclear weapons. However, it turned out that the controlled principle of thermonuclear fusion aimed for harnessing energy and the 'uncontrolled' principle of thermonuclear weapons aimed for mass destruction were essentially disparate approaches; a fact that made the preliminary fusion research fundamental in character and mostly trial and error-oriented. This eventually resulted in complications of such intricacy that, in the latter half of the decade, research declassification became widely necessary, as noted above. Then again, despite all pessimism, those countries involved in fusion research would keep on expanding their knowledge bases of accumulated intellectual capital on which they drew to continue their efforts; the learning obtained from failure most certainly can be valuable in this perspective. And, together with the official research declassification at the Geneva Conference in 1958, a new chapter of knowledge distribution and exchange was begun, improving the prospects for a growing international community. I find this reasonable bearing in mind that a scientific community's knowledge base can be linked to David's perspectives on *complementarity*. International research declassification in fusion enabled both collaboration and competition among fusion actors on the basis of open information, hereunder the sharing and mutual utilisation of each country's accumulated knowledge base. In this way the Americans could learn from the Soviets, and vice versa; a key to the positive externalities that would enhance the work of each country and thus the scientific community as a whole.

Furthermore, the 1950s saw the emergence of different technological approaches to the attempt of solving the problem of controlling fusion reactions. All methods were heavily experimental, yet potential fusion reactor designs with variable performance, weaknesses and strengths. Curiously enough, the designs officially presented at the 1958 Geneva Conference derived for the most part from similar principles, i.e. set-ups performing as either open or closed systems with steady-state or pulsed-mode operation. This indicated a certain paradigm or trajectory already at early stages. Then again, the fact that newer, useful theory was scarce, making the rudimentary fusion technologies generally linked to proven theoretical physics, might reveal the similarity of designs as self-explanatory. Therefore, the significance of early *accidents* affecting the course of future designs and research was highly owing to the trial and error-approach commonly used by fusion scientists and engineers; no one knew whether or not the next test would be useful; a reality which applies to David's emphasis on *coincidence* when discussing technological evolution and the emergence of trajectories in fusion. Indeed, under circumstances of science where uncertainty is a prevailing dynamic, coincidental events during experiments may have critical implications for future proceedings.

3.3. Phase 2: 1958-1969 – collaboration, competition, and dominance

3.3.1. Political framework

The political conditions related to fusion in the 1960s were for the most part similar to those of the preceding decade. However, the research would undergo periods of adversity marked by severe funding issues, especially in the United States due to stagnation of progress in fusion at the same time as the overwhelming programmes in space and nuclear weapons research were claiming the majority of interest and budgets; research for peaceful energy uses was clearly not of immediate priority. Parenthetically, the 1968 International Atomic Energy Agency (IAEA) Conference in Novosibirsk (see below) came to pass at a time when the international political strain was at its lowest since the beginning of the Cold War, and the 'Prague Spring' had manifested itself even at the university campus where the conference took place (Braams 2002, p. 134).

3.3.2. Scientific community

As a consequence of the distribution and exchange of knowledge that was first facilitated at Geneva, the laboratories now shared their information, enabling both partial replication and reverse engineering. Princeton, with its winning stellarator concept, no longer held monopoly on the machine's architecture or experiments. True, the laboratory had benefitted from the lead-time of having constructed a series of devices early on, but now the personnel at the Lebedev Institute of Moscow, and later several other laboratories, were catching up with it[30] (ibid., p. 125). The research on tokamaks, on the other hand, did not experience matching popularity. Until the revolutionary tokamak performance results were presented at the IAEA Convention of 1968 in Novosibirsk, the work on tokamaks was executed by the Kurchatov team alone. The pursuit of the tokamak line was in fact of negligible interest to other institutes outside or inside the Soviet Union, with the exception of the Australian National University in Canberra where pinch experiments were done with configurations reminiscent of the tokamak

[30] The stellarator vs. tokamak-period comprised both international and domestic competition in this regard.

as early as in 1965 (ibid., p. 130). In fact, the prospected tokamak design described an energy source which massive physical dimensions and low cost-effectiveness received nothing but immediate rejection from the United States and others (Furth 1990, p. 3). To begin with, at the IAEA 1965 Culham Conference the Soviets presented portions of their tokamak performance results for the first time. In short, these were exceptional and superior to those of competing concepts such as the stellarator, but the Kurchatov team was nonetheless only to be distrusted (Bromberg 1982, p. 151). After all, customary Soviet deductions in fusion research had become founded chiefly on empirical methods, e.g. by making repeated observations of the plasma's behaviour in experiments and taking cues from this: the Soviets were not so much concerned with analysing every *theoretical* detail of tokamak physics as simply pursuing what seemed to work (Herman 1990, p. 98). This – to the Americans especially – unorthodox and 'untheoretical' strategy allowed scepticism to continue. However, at the 1968 conference in Novosibirsk the Soviets would present results that were even more impressive, and although the Princeton staff still expressed doubts concerning the measurements, they could simply not ignore that the Moscow team had indeed entered a promising course towards thermonuclear conditions. The newly produced information had to be investigated in a more reliable fashion (Bromberg 1982, p. 152); an operation that would be carried out with the help of cutting edge diagnostic techniques solely commanded by the American and European fusion scientists (Fowler 1997, p. 26)

On paper, it was possible to make direct measurements of plasma electron temperature by directing intense light, more specifically lasers, on the plasma current and then observe its scattering pattern to accurately determine a number. Spurred by this, Artsimovich and British physicist R. Sebastian Pease had arranged for a unit of the latter's own Culham Laboratory specialists in scattered laser measurement of plasma properties to head to the Kurchatov Institute (Bromberg 1982, p. 167). This was to be the first of quite a few occasions of international cooperation involving the sharing of actual equipment, as well as expertise and skills, within the fusion community (Fowler 1997, p. 26). The British unit transported its five-ton laser hardware to Moscow in the spring of 1969 during what has been later known as the Culham expedition, and set it up on the T-3 tokamak installed there to re-measure its temperature. By August, the unit had acquired preliminary, reproducible results. These were conveyed back to the UK, which in turn made a confidential phone call to Washington: the Soviets had been correct about the 10-million-degrees performance of the T-3 all along, as well as the plasma density, and it was all now authenticated by an external party's complementary involvement (Bromberg 1982, p. 164).

3.3.4. Technology

In the decade following the 1958 Geneva Conference, the stellarator and tokamak would become the undisputed mainstream toroidal systems and competitors in the race for attaining the best results in terms of the Lawson criterion (Braams 2002, p. 124). Although the stellarator was the one most prominent, I will concentrate on the progress of the tokamak. Since the earliest reports on tokamak concepts in the early 1950s (then called toroidal pinches with strong stabilising fields) and the post-Geneva 1958 toroidal construction named T-1, the Moscow Kurchatov team experimented with a succession of tokamaks of approximately the size of T-1. In addition to the actual characteristics of the machines, the Kurchatov team steadily improved their array of diagnostic instruments and controlling mechanisms so that by the time of the Culham Conference in 1965, the key qualities of standard tokamak plasmas had emerged. With models T-3 and TM-3, the team advanced the tokamak programme to a remarkably sophisticated level, however one that was fully acknowledged only after the 1968 IAEA Conference in Novosibirsk (ibid., p. 129, 132). The conference turned out to be a success for the Soviet arrangers in terms of the mass attention being directed on the most recent and impressive performance results of their tokamaks (ibid., p. 152). This was sorely needed, considering the wide neglect for tokamaks before. Then again, the abovementioned disagreement on different ways of measuring the electron temperature ensued as the values of temperature, in addition to density and confinement time, that were alleged by Artsimovich for the T-3 and TM-3 models had radically surpassed the parameters yet recorded on any other toroidal device, or in fusion generally, for that matter. The electron temperature claimed was the then-astonishing value of 10 million degrees Celsius, the value Princeton researchers had earlier worked towards but not yet achieved. Thus, with the Culham expedition verifying these numbers, little could prevent the 'tokamak revolution' from taking place: the Soviet technological sensation was made the primary focus of fusion research henceforth, and is still the standard by which progress on other fusion concepts is evaluated.

On the whole, at the end of the 1960s both plasma theory and experiment had assumed a certain degree of scientific maturity and were displaying increasing accord; recurring instabilities and obstacles related to both plasma physics and machinery were diminishing. Indeed, the time seemed ripe for a change in philosophy, namely for plasma physicists to direct their attention once again to a more applied route: the building of a thermonuclear reactor (ibid., p. 134).

3.3.5. Traits of path dependency

In the United States, the 1960s displayed a field of research that was struggling to move forward, and to be properly seen and heard by the government. Although the declassification in the preceding decade had bettered the potential for success, and although the stellarator was regarded as the most nascent among the competing approaches[31], American fusion research was showing unsatisfactory progress, and was punished accordingly by low governmental priority. It is therefore plausible to treat the Soviet work on the tokamak as the principal driving force of the fusion community in the 1960s. Hence, I hold that the turning point of high relevance to path dependency was the Culham expedition in 1969. This operation stands as an example of how *technical interrelatedness* and *coincidence* have worked in sympathy, and heavily affected a research field's particular trajectory. First and foremost, by combining expertise and first-rate technology, i.e. the Soviet tokamak T-3 and the British scattered laser measurement, the performance of the tokamak could be translated by means that were debatably more accurate than the Soviet diagnostic instruments, and thus more trustworthy. *Complementarity*, then, is evident as the two technologies were mutually augmented: the scattered laser system as a sophisticated and highly influential measuring tool for future use, and the T-3 as an indisputably groundbreaking tokamak that provided a radical step forward for fusion; results were from now on far better *and* more reliable. There is, on the other hand, debatably a bias factor present in this incident. Given that the Soviets had been correct about the T-3 performance ever since they first presented its results at Culham in 1965, I find it reasonable to contend that the forces responsible for the tokamak to be downplayed until four years later were just as social as political, scientific, and machine-related. The American and British scepticism towards the Soviet findings was dissolved only in the wake of their own participation, signalling the importance of prestige as well as the somewhat negative (tactical) implications – in effect, working *against* innovation – that may occur in an open competition environment. Also, observers have located a thought-provoking detail which demonstrates the impact of coincidence on the tokamak-story; one that rejects the misleading notion of path dependency as a deterministic explanans (albeit its narrative form may occasionally appear as such). After the T-3 success, Soviet researchers continued their work on the other cutting edge tokamak presented at Novosibirsk, TM-3. When studying significant portions of data derived from the machine it was discovered that critical electron values were comparably poorer than

[31] When considering non-scientific factors such as cost-efficiency and sustainability.

those of the T-3, and not in compliance with a dominant approach. Princeton scientists have therefore reflected on what might have been the outcome of the Culham expedition if the original laser measurements had been made on the TM-3 instead of the T-3 (Bromberg 1982, p. 167). The tokamak could well have never come to pass. All the same, the fact remains that the Soviet tokamak made a powerful contribution to *path validation* in fusion. In retrospect, one may point out that this achievement started at the Novosibirsk conference with the presentation of the T-3 and TM-3 data which eventually pushed fusion technology into a more defined research trajectory with the subsequent tokamak controversy and ultimate success. The circumstances of the Culham expedition additionally brought the initiative for organised international scientific collaboration to the forefront of new strategies[32] (Braams 2002, p. 134). Furthermore, the Culham expedition endowed a positive impact on the climate for East-West collaboration that soon culminated in bilateral agreements drawn up between the Soviet Union and the United States, and also between the Soviet Union and the principal European nations participating in fusion research; agreements that assisted frequent exchanges of personnel among the fusion laboratories (Fowler 1997, p. 27).

3.4. Phase 3: 1969-1971 – worldwide switch to tokamaks

3.4.1. Scientific community

After the Novosibirsk Conference, the 'tokamak revolution' became visible at the beginning of the 1970s as a decisive turn to tokamaks occurred in several laboratories in various nations working on magnetic plasma confinement (Shafranov 2001, p. 2). In this regard, a notable geographical exchange would transpire: the Soviet Union, ironically, renounced the forefront of tokamak research as they chose to focus their work on the largest stellarators in fusion instead (although maintaining a solid expansion of their tokamak series), while the United States temporarily shut down all stellarator activity in order to construct exemplars of the largest and most renowned tokamak specimens today (Braams 2002, p. 143). I.e. the Soviet tokamak approach to fusion had not only evolved to dominance, but it would become widely adopted by former competitors abroad as well.

[32] The contacts made in this respect turned out to be considerably rewarding and able to overcome adverse political and scientific circumstances in the future (Brams 2002, p. 134).

a. Notable activities

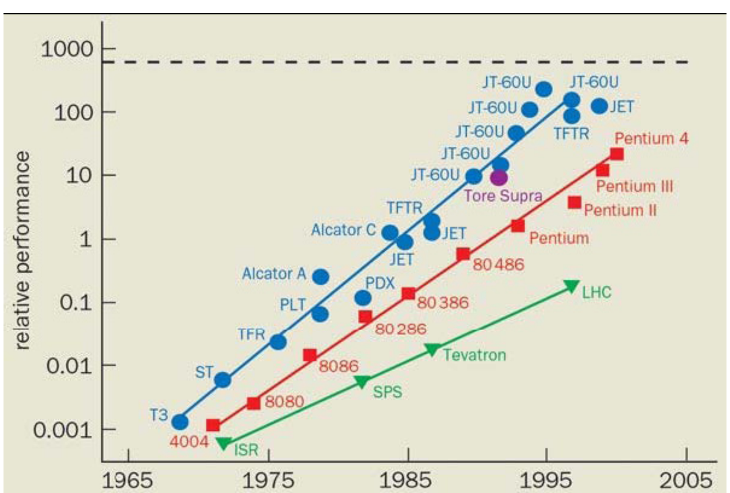
Members of the recently formed Controlled Thermonuclear Reactor (CTR) Standing Committee, which included heads of the fusion divisions in the major US laboratories, were eager to outstrip the Soviet tokamak efforts by specifically taking on technological directions different and more innovative than these (Bromberg 1982, p. 168).The Massachusetts Institute of Technology (MIT) instigated construction of the first in the Alcator series of high-field tokamaks, and Princeton converted their latest stellarator model (C-stellarator) into a full-scale tokamak called the Symmetric Tokamak, and also built a smaller tokamak type for auxiliary tokamak-grade plasma studies. Oak Ridge, having been devoted primarily to mirror machines in the past, also entered the tokamak bandwagon with their ORMAK I installation. At San Diego, the General Atomics laboratory initiated an original line of a particularly elongated, unorthodox tokamak called The Doublet (Braams 2002, p. 153). In Europe, Japan, and other countries, the similar movement took place. France, being the most intrepid newcomer, built their massive TFR (Tokamak Fontenay-aux-Roses) without the support of preliminary works. The German fusion laboratory in Garching built a smaller tokamak device, Pulsator, and the Italians at Frascati built the modest TTF and the high-field Frascati Tokamak (FT). The British party at Culham interjected the construction of their long-awaited new stellarator CLEO by modifying it into a tokamak. CLEO was operated in 1971-73 while their primary purpose-built tokamak DITE was being assembled. In Japan, two tokamaks, JFT-2 and DIVA, were constructed at the Japan Atomic Energy Research Institute (JAERI) site near Mito and others at Nagoya and Tokyo (ibid., p. 156). In the tokamak homeland, Soviet tokamak research was continued with an increasing and varied programme of machines. The T-3 model was restructured with enhancements made to the electrical current capacity, while the next additions of T-4 and several other new tokamaks were built to study specificities and micro-issues. T-5 provided the experimental verification of main theoretical predictions concerning plasma equilibrium. Substantial modifications such as the changing to vertical magnetic field and no copper shell, was demonstrated in the TO-1 tokamak; a technique adopted for the majority of successive models (ibid., p. 153). Over time, not only did the 'tokamak revolution' see most institutes for fusion research dedicate parts of their efforts to tokamak physics, but this also included a number of university laboratories[33] (ibid., p. 156).

[33] By 1986, the IAEA had registered in excess of 70 tokamak installations worldwide, some of which were minor devices made for teaching purposes.

3.4.2. Technology

In the United States, the years from 1968 to 1970 carried a renaissance of optimism among the members of the American fusion community after a decade of dejection due to unsatisfactory performance of both stellarator and mirror installations, in combination with financial hindrances. As mentioned, the optimism was triggered in some degree by advances in their existing technologies, but above all the tokamak, which gained access for toroidal devices into completely new regimes of plasma parameters. The significance of these regimes was fundamental; one of the guiding rules in CTR strategy was the realisation of plasma conditions as close as possible to those in reactor conditions, which was very much obtained by the recent tokamak figures (Bromberg 1982, p. 172). Starting at 1970, the next three decades would establish the tokamak research as the genuine mainstream direction within fusion and fusion in general, evident in continuously increasing scales and near-exponentially growing performance levels (see figure 9) sustained by a rapidly accruing data base, enabling the design of still larger machines. In the 1970s, this growth pattern soon led to a capacity breach at all but the largest institutes and would eventually exceed the capacity of the national programmes as well. Thus, the design studies that were set off in the early 1970s first required reinforcement of national, and within Europe international[34], coordination. In the following stage, limits of national capacity eventually resulted in a call for global cooperation (Braams 2002, p. 157).

Figure 9. Fusion experiments have kept pace with other hi-tech developments (source: Bergh 2005, p. 17)



[34] Through the EU.

Note: Since the early Russian T-3 tokamak, the performance of fusion plasmas has doubled every 1.8 years (blue line). The performance of fusion plasmas is defined in terms of the triple product (density × temperature × confinement time). This triple product compares favourably with the doubling of the energy of particle accelerators every 3 years (green line), and the doubling of the number of transistors on a chip every 2 years (red line). The dashed line at the top shows the performance expected with ITER (Bergh 2005, p. 17).

3.4.3. Traits of path dependency

I hold that the 'tokamak revolution' was in effect synonymous with the primary phase of the tokamak's budding *momentum*. Owing to the adoption of tokamak technology in fusion science centres worldwide, the field of fusion would grow from technological homogeneity as multiple developers behind a single developing target enabled a more comprehensive and rapid learning process. In terms of path dependency (and later on, ITER research lock-in), this was another aspect of *complementarity* in that the value of a good (i.e. tokamak) increases with the size of its installed base (i.e. fusion scientists and engineers, governments, etc.) Hence, the nascent reactor concept was subjected to a self-reinforcing course in that its scientific characteristics were distributed across an extensive range of countries and country-specific resources; human skills and facilities that could be fully exploited in advancing the tokamak further. Moreover, I argue that the adoption process was a consolidation of width and depth which induced what would eventually transpire as a phenomenon closely linked to David's *inertia of sunk costs*: the inertia of mass adherence to a specific scientific approach. In other words, with the widespread effort on tokamaks, an international research infrastructure built on tokamak standards was emerging, essentially making up a practical fundament prepared for joint international tokamak efforts such as ITER. This necessarily implied that less acknowledged concepts, e.g. stellarators or mirrors, would turn out rather impractical for employment in similar ventures should they prove more feasible than the tokamak at some point in the future.

3.5. Phase 4: 1971-1985 – larger machines

With the tokamak being "the winning line", the Soviet Union openly discussed further plans to design a new generation of drastically larger machines in their T-series, starting with the T-10, and aimed to complete the construction of the final model T-20 within the end of the

1970s. In the wake of T-3, the West had reacted with alarm and a revived ambition of its own, and this was also the case when the long term T-series scheme of large machines was publicised; the focus on magnitude became the next stage in the fusion community altogether (Herman 1990, p. 99).

3.5.1. Political framework

Controversy and confusion about resources (energy and materials) and environment became frequent political focal points during the 1970s. The Arab oil embargo and subsequent oil crisis in 1973, and the Reports to the Club of Rome from 1972 to 1974, had accentuated the global dependence on fossil fuels. This, adjoined by the emerging discussion on climate issues, created a substantial incentive for further promotion of the inherently emission-free fusion energy production (Braams 2002, p. 240). Thus, the 1970s granted more favourable conditions for fusion research in the United States than had the previous decade, and among the alleviating mechanisms were indeed shifts in the political framework. During the 1960s, the American fusion programme had remained under firm pressure from Congress to upgrade technological performance and improve plasma parameters in order to earn raises in budgets as fusion was of marginal priority compared to parallel science projects, e.g. the space programme. The long-awaited progress was now coming about, chiefly owing to the tokamak success. And perhaps more importantly, in the 1970s United States, the science of fusion would attract both public and political attention to the challenges of future energy demands and environmental issues, and how they could be possibly addressed. An overarching concern regarding pollution had grown sharply in the middle and late 1960s, culminating in the nationwide, grass-roots celebration of Earth Day on April 22, 1970. Congress responded on this with the National Environment Policy Act of 1969 and the Clean Air Act of 1970. Later that year, President Nixon brought together federal pollution-control agencies into the new Environmental Protection Agency. These implementations were not unproblematic as a perceived growing shortage of energy had been coinciding with the environmental movement, and paradoxically seemed to be in conflict with the need to purify the atmosphere and the watercourses. An example of this predicament was a severe power failure during the second half of the 1960s. The incident had afflicted the Northeast, proceeded by a sequence of smaller blackouts and a generally reduced electricity supply (Bromberg 1982, p. 175). The request for more electricity was genuine, and fusion, an intrinsically emission-free method of producing energy, thus served as a promising vassal for the securing of abundant power supplies and a cleaner alternative to polluting large-scale technologies.

3.5.2. Scientific community

a. Europe

Fusion research in Europe had originated in five out of the six original member states of the now called European Union (EU). From shortly after the Geneva Conference of 1958, coordination was directed by arrangements under the Euratom treaty. These arrangements were also responsible for partial funding. With the EU-admittance of the United Kingdom and Denmark in 1972, both countries having their own fusion programmes, the Euratom fusion research was strengthened considerably and became more able to accomplish the construction of a large-scale European tokamak. Discussions on building a device bridging the gap to a future prototype fusion reactor ensued, but quickly revealed that a project of this magnitude would be beyond the budget of a single national laboratory. Hence, a prospect for a European collaboration was proposed and subsequently acknowledged. It was agreed to establish a multinational project team to design the big machine which became known as the Joint European Torus (JET). The JET design team published its final proposal in 1975 and Culham was eventually selected as the tokamak installation site. JET was to be the foremost experiment of the European Fusion Programme, and its organisational structure, a Joint Undertaking under European law, was formally established for a tentative duration of 12 years[35] beginning on 1 June 1978. The EU budget would carry 80 percent of the costs, non-EU participants such as Sweden and Switzerland would carry 10 percent, and the United Kingdom, benefiting from local expenditure as the host of JET, would pay a special contribution of the remaining 10 percent. Hans-Otto Wüster, a German physicist who had managerial experience from his work at CERN, was appointed director of the project. The assembly of JET facilities at the Culham site commenced in 1979, and the final construction stage was completed in January 1983 (Braams 2002, p. 206).

[35] JET operation was prolonged several times during the 1980s, 1990s, and 2000s.

b. The United States

An important feature in the American commissioning of large-scale tokamaks was the change in the institutional structure with the transition from a laboratory-level strategic management to an increasingly federal format and higher echelon of command. Since 1970, the country's fusion programme had entered a continuing process of centralisation where it went from being loosely organised to more resolute and governed by a concrete test reactor goal. The political determinants of programme strategy would no longer be restrained solely among the fusion scientists and engineers, but involve more of the government and the public at large. As a result, a significant number of technically trained persons were brought into the Washington office; men and women who assumed important positions on the various peer-review committees. The new Washington staff would take on decision-making functions that had formerly been executed internally in individual laboratories, and also advisory functions that were traditionally vested in the laboratory directors (Bromberg 1982, p. 216).

The European decision in 1973 to design a big tokamak, besides the Soviet intentions with the larger T-series models, put major pressure on the Americans (Braams 2002, p. 206). American designers estimated that a machine in the United States able to surpass the Soviet T-10 venture could cost several tens of millions of dollars. The construction and operation of such an installation would require personnel of hundreds of individuals: a sizeable team of physicists aided by crews of engineers, computer experts, financial administrators, and support staff. Briefly put, a tokamak of this size and complexity would require a bureaucracy. What is more, the new machine would consume extensive loads of electricity to activate each test, and operation and maintenance costs would by far exceed the numbers of any project at any United States fusion laboratory in the past (Herman 1990, p. 99). Even so, determined to maintain a leading position, the Americans commenced a conceptual design study of a large experiment early in 1974 which produced a documented blueprint for the Princeton-based Tokamak Fusion Test Reactor (TFTR) two years later. Scientific, technical, and commercial feasibility would become the necessary milestones on the scheduling charts of research administrations in order to justify their growing budgets. Hence, the TFTR objectives were to display fusion energy production from the initially pulsed-based burning of deuterium and tritium in a magnetically confined toroidal system. This to examine the physics of large tokamaks, and to attain essential experience in the solving of engineering problems associated with fusion systems approaching the dimensions of experimental reactors (Braams 2002, p. 206). By 1974, with its boosted funding and substantial plans for the TFTR, it is stated that the American fusion programme crossed the 'Big Science threshold' (discussed below), as had Europe with JET (Bromberg 1982, p. 216). Construction of TFTR started in April 1976 at Princeton which had become the most comprehensive fusion laboratory in the United States and, with the smaller tokamaks PLT and PDX already under way, had abandoned stellarators to commit fully to the tokamak line (Braams 2002, p. 206-207).

c. Japan

Alongside the European JET and the American TFTR, the third of the initial large tokamaks would be developed in Japan. The machine was to be called the Japan Torus-60 (JT-60). Fusion research had been of high interest in the country since the 1955 Geneva Conference, but it was decided that preliminary scientific activities should maintain a basic nature and reside under the Ministry of Education (ibid., p. 207-208). The traditional Japanese culture of consensus among interested parties was considered vital to success also in fusion. Following the global research declassification in 1958, when other countries rushed to work on experimental machines, Japan spent a year in national debate on the most fruitful strategy (Herman 1990, p. 110). This was before the Japan Atomic Energy Research Institute (JAERI), under the Ministry of Trade and Industry, became involved. For this reason, the first phase of the fusion programme at JAERI, represented by the construction of the small JFT-2 tokamak, did not commence until 1969 (Braams 2002, p. 209). Despite the conservative approach of the past, however, Japan would advance relatively swiftly into the world of bigger machines once research had gained momentum. Having learned of the West's ambitious plans to construct large-scale tokamaks, and due to the concurrent oil embargo in Arab nations, the Japanese considered taking the same intrepid leap from their own modest test devices to a much larger tokamak. Once again, decisions were founded on a national consensus among scientific, political, and industrial leaders (Herman 1990, p. 106). Party to this second debate was fusion scientist Tadashi Sekiguchi. He has stated the following:

> Japanese economics was very upset by the first oil shock of 1973….We realised our energy situation was very fragile. It could easily collapse. Importing raw materials of all kinds, making goods, adding value and exporting is the only way Japan can live (ibid., p. 111).

Japan was acutely aware of its vulnerability concerning future energy supplies. In this regard, fusion presented both a grand opportunity for the country and a considerable risk due to its expense and the speculative state of the science. In late 1974, after much deliberation, the Japanese Atomic Energy Commission elevated fusion to the prestigious status of a 'national project', put beside space exploration, ocean development, and nuclear fission technologies (ibid., p. 113). The second phase of the JAERI programme, beginning in the same year, contained proposals for the JT-60. The machine was to be built on a new site at Naka, proximate to the main JAERI laboratory. It went into operation in 1985. Consequently, Japan entered the league of large devices with a portfolio of comparably fewer intermediate-sized tokamaks than Europe and the United States (Braams 2002, p. 209). On three continents, a common understanding of fusion as Big Science, in Weinbergian terms, was emerging.

d. The Soviet Union

Interestingly, during the 1970s, the Soviet position in fusion seemed to be pining. Where once they had dominated the tokamak hardware competition with their primary expertise, their plans to build the large-scale T-20 tokamak (with superconducting magnets) would ultimately be abandoned owing to budgetary restrictions. Their fallback was a more moderate-sized superconducting device called T-15, but it had been delayed for years due to complications in construction. This signified that the Soviet Union was left without a modern tokamak. The revered T-10 model which had worked as a catalyst for other countries' large-scale initiative in fusion had eventually proven to be an empty threat. It was revealed that its characteristics were similar to those of the American PLT device, only less sophisticated and run by the same, eventually outdated, computer used during the Culham expedition. It is argued that the Soviets were still the world's most formidable fusion scientists – that is, with pen and paper. The dimensions and expenses of bigger tokamaks, on the other hand, demanded more and put the Soviet Union at a disadvantage (Herman 1990, p. 167-168).

3.5.3. Technology

The soviet theory behind larger tokamaks asserted that if they could make T-20 work successfully, it would enable the Soviets to achieve reactor-grade plasma conditions and finally prove the scientific feasibility of fusion power. This prognosis was made on the foundation of their confidence in size which is derived from a simple application of physics called 'scaling laws' (ibid., p. 99). The single parameter that is closest to determining the dimensions of a tokamak reactor is the plasma discharge (Braams 2002, p. 202). Scaling laws, then, correspond to the confinement time variable of the triple product, postulating that when plasma is expanded inside an enlarged magnetic chamber it will take proportionally longer before it comes into contact with the cool edge of the chamber wall and terminates. This way, the plasma discharge will exist longer and thus better the probability for ignition to occur. Physicists were positive that changes in scale would reap major gains. For instance, a two-factor increase of plasma radius was expected to yield at least a fourfold improvement in confinement time. In fusion, with the tokamak at the technological vanguard, big was better (Herman 1990, p. 99). However, the scaling laws implicated more than a greater physical size of the tokamaks – equally greater amounts of money were required to implement and support it.

3.5.4. Traits of path dependency

The influence of environmental issues and energy-related concern on policy clearly worked in favour of fusion in the early 1970s, especially in the United States where the contradicting demands for large-scale production of energy and reduction of pollution posed a dilemma to which fusion solutions seemed almost tailored. As in the 1950s, the political agenda of fusion countries had once again become *compatible* with fusion research, and this time around, compatibility was based exclusively on peaceful uses and the benefits of a far more mature field of science; the development of a defined, technological concept (tokamak) that had the potential of being transformed into reactor-grade dimensions seemed particularly inciting to governments. Without a doubt, the change in strategy from basic research to a more applied focus tied to actual reactor designs did make for greater political appeal. Accordingly, with refurbished governmental support, the scientific and technological progress of fusion would face less managerial and budgetary friction following the tokamak's momentum. In fact, an evident cause of the tokamak actually acquiring this *momentum* was the decision to redirect research towards the construction of test reactors, and hence entering the stage of larger tokamaks. The escalation of size initiated the professed bureaucratisation of fusion, with increased funding and addition of participants, personnel and delegated functions (notably administration), implicating a more formal level of organisation and centralised regulation. In Europe, this led to the first official setup of international cooperation in fusion, establishing a European Joint Undertaking through Euratom with the JET device. On account of the mounting requirements for skills and technology, and associated expenditures, the scientific growth of fusion would exceed the capacity of single nations. Accordingly, the rationale of JET was to facilitate sustained fusion research with the realisation of next generation tokamaks through inter-European sharing of labour and costs.

With this in mind, I argue that large-scale research projects and formal international cooperation are valid mechanisms in terms of David's *economies of scale*. I choose to utilise David's notion in this context to illustrate another early manifestation of research lock-in in fusion. It seems plausible to assume that there is a negative correlation between the fixed costs of operating specific research projects such as JET, TFTR, JT-60, and ITER, and the number of participants involved in the project. Since a fusion research project is by nature a non-profit endeavour in the conventional sense, I rather propose the idea that the returns of research expenditures are measured by the quantity and quality of units of knowledge produced, representing the scientific equivalent to revenue. Moreover, the division of labour and specialisation that follows with large-scale projects is central to this relationship, seeing as project personnel is organised to carry out defined and interdependent assignments that both ensures focus and specialised training. Over time, this organisational structure may enhance research efficiency. Thus, for tokamak projects, the logic of economies of scale postulates that when the size of machines and specialised personnel is expanded (e.g. an added number of people working on a single research project through international cooperation) the value of the project becomes greater due to more and better results relative to fixed project costs. Such dynamics are only obtainable at a certain scale. Consequently, I maintain that size would intrinsically serve to further reinforce the path-dependent pattern of the tokamak evolution as the mechanisms of economies of scale and cooperation helped to overcome the constraints of limited national funding and resources, and to enhance durable research performance within the tokamak domain. This applies to both the American TFTR and the Japanese JT-60, but first and foremost to the European JET installation with its inter-European model.

Hence, it follows that the Soviet concentration on scaling laws provided a technology-oriented stimulus for making larger tokamaks in the 1970s; one which has also worked to support tokamak research lock-in in the long run. This is evident as the period starting with the construction of the first large tokamaks shows research programmes taking on a gradually escalating size in conjunction with incremental innovations directly or indirectly related to the machines. And it is the wealth of experimental data gathered from these activities that validates the building of reactors to the ITER design specifications. It is held that, if done correctly, there is little doubt that the scientific goal of a sustained thermonuclear burn will be accomplished, but this learning empirically confirms the implication of the scaling laws: tokamak dimensions must be brought to a certain scale in order to contain proper conditions for energy break-even and ignition (Glass 1998, p. 1).

The next step

This concludes the history analysis chapter as the next step is the actual establishment of the ITER project which quickly directs our focus to the more recent problems that are presented in chapter 1, hereunder the final part of the research problem. The birth of ITER, then, will be treated below as an introductory part of the main analysis chapter. I have chosen to arrange it this way seeing as chapter 3 has been used to single out the growth factors in fusion, and how the science has gone from being an ambiguous newcomer of classified and discrete activities to an internationally interconnected discipline; one that is aiming for a completely new energy source largely based on a distinct technological approach with the tokamak.

Chapter 4. Main analysis

4.1. International cooperation

As a bridge between the history analysis and the main analysis section, I will give a résumé of the circumstances proximate to the 1985 ITER inception, and the final pre-ITER discussions per se; this to denote the fully international culmination of the long-time growth of tokamak fusion and the move to the main analysis of this thesis.

As shown, international linkages in fusion research have a prolonged history. They originate from the classified wartime agreement between the United States and the United Kingdom, and entered the public domain with the Soviets' promotional visit to the United Kingdom in 1956. Through successive bilateral and multilateral arrangements, fusion physicists developed a close worldwide network, constituting a beneficial ground for true international cooperation later on. In this respect, since the early 1970s, nuclear engineers have teamed up and produced a number of reactor design studies, yet with a variable degree of credibility. In 1977 the Massachusetts Institute of Technology (MIT) hosted a meeting between senior engineers from different countries to discuss how their fusion programmes could be better integrated to further the overall credibility of designs. Two international agencies proved to be potentially suitable for this: the IAEA, under the auspices of the United Nations, and the International Energy Agency (IEA). The fusion community, however, was determined not to exclude the long-time Soviet colleagues from the integration efforts, and was reassured when Soviet leaders themselves proposed to set up a workgroup under the IAEA to consider the circumstances of a common fusion research endeavour (Braams 2002, p. 247).

4.1.1. INTOR

The workgroup team, named INTOR (International Tokamak Reactor), would explore the 'next step' tokamaks from the JET, TFTR, JT-60 generation. The sessions were conducted through a network of general and specialist 'workshop' meetings supported by domestic activities in each of the participating countries, aiming to bring together tokamak reactor studies in two ways. 1.) National design teams performed organised, comparative analyses of their reactor studies and incorporated the learning from this information exchange in their own models. 2.) INTOR unified physicists and engineers as real representatives of distinct fusion programmes, and not merely ad hoc participants as in previous minor reactor studies. In a series of reports INTOR accumulated a database of pertinent scientific and engineering information. This was utilised to compose a conceptual design for a reactor with the capacity to display required plasma performance and establish the technological feasibility of fusion power generation. Moreover, in parallel with the INTOR studies, the four participants worked on their own concepts of 'next step' devices which could replace the common project lest it would not be carried out. During the final INTOR sessions, the four alternative designs were evaluated and compared. In 1979, INTOR had reviewed the literature on existing empirical scaling numbers for plasma confinement and proposed its own specific scaling. This functioned both as the basis for the INTOR design and as a point of reference against which the results of ongoing experiments could be assessed (ibid., p. 248).

In due course, though, tokamak physics in the early 1980s would encounter new heating-related obstacles which made the knowledge and techniques at the time fall short with respect to INTOR's conceptual design. Furthermore, these issues disclosed differing interests, perceptions, and expectations among the four participants towards an actual test reactor. INTOR came to an end. In hindsight, it is apparent that the INTOR database assessment and conceptual design, along with the four domestic studies, assisted the focus of tokamak physics and fusion technology research into areas of reactor relevance, but progress was evidently not mature enough for an attainable joint effort (ibid., p. 249).

Nevertheless, a valuable international project outline had been sketched for the imminent future. All in all, INTOR was an effort to systematically coordinate and unify international fusion research, and can therefore be regarded as an 'organisational prototype' of ITER. In the light of budgetary restrictions within the general fusion community at the end of the 1970s and early 1980s, this prototype, notwithstanding its letdown, was a preparation to a fully implemented international effort.

4.1.2. ITER: international cooperation formalised

The actual internationalisation of fusion occurring in the United States, through the formal ITER Agreement, can be traced to Washington in 1981 with the arrival of Alvin Trivelpiece as director of research in the United States Department of Energy. This was shortly after the passage of the Magnetic Fusion Engineering Act of 1980, signed into law by President Carter before leaving office. This act had required extensive increases in fusion appropriations and the construction by 1990 of a fusion engineering test facility similar to what later became ITER, but run and funded by the Americans alone. Needless to say, this act was never implemented. With the entry of the Reagan administration, energy issues seemed to be eliminated from the federal agenda, despite the concept of 'energy independence' having been promoted by Republican presidents Nixon and Ford in the past. Energy independence was to be substituted by reliance on the international marketplace for oil. The Reagan administration even went to the extent of planning to abolish the Department of Energy instated by Carter earlier. Trivelpiece, diverging from the overall administration's new agenda, had convened President Reagan's science adviser and the head of the National Science Foundation to assess the situation for research. From these meetings and his own investigation, he had become convinced that the only chance for retaining a viable fusion programme in the United States was in pooling expertise and resources in a fully coordinated international endeavour (Fowler 1997, p. 113). T. Kenneth Fowler, Oak Ridge Laboratory professor in plasma physics and magnetic fusion energy, elaborates:

> I soon found myself among his [Trivelpiece] accomplices in this enterprise. I recall in particular a meeting on international science projects that Trivelpiece and I attended in Copenhagen. There I learned that throughout the world Big Science was grappling with the same questions of how to move forward with costly projects, and that, like Trivelpiece, most science administrators suspected that in the future individual nations would no longer be able to handle most such projects (ibid., p. 114).

Trivelpiece had arrived in Washington with especially strong credentials to guide fusion research into new directions. Then again, not only did he need to persuade his fusion colleagues to put international fusion cooperation into practice, he was also dependent on a favourable political framework. It eventually turned out that French president Mitterand's Versailles Group of Seven (G7) summit would provide this framework. Mitterand's agenda was resting heavily on technology development, with fusion prominent on the list. While several members of the American fusion community attended frequent meetings in the United States, Europe, and Japan from 1980 to 1985, the internationalisation project would gain little headway against the remaining people who still preferred smaller, domestic proposals (ibid.).

In the meantime, General Secretary Mikhail Gorbachev had risen to power in the Soviet Union, and came to present an alternative and unexpected opening. Travelling with Gorbachev on his peacemaking visit to Europe in 1985 was Evgeniy Velikhov, a fusion scientist and director of the Kurchatov Institute. He was also a man of political importance being a close adviser to Gorbachev as elected member of the Supreme Soviet and vice-chairman of the Soviet Academy of Sciences. Velikhov's outspoken advocacy for the concept of international coordination in fusion hence provided beneficial influence. Accordingly, the first summit meeting in Geneva between Reagan and Gorbachev finally offered the channel that Trivelpiece had been seeking to instigate a truly international effort in fusion. The initiative came from the Soviets through Velikhov, assisted by Trivelpiece, working through the US secretary of energy and the Department of State (ibid., p. 115). After much debate, the United States and the Soviet Union agreed to advocate "the widest practicable development of international cooperation" through fusion (Smith 1985, p. 3). Their concluding communiqué announced that their countries, joined by others, would collaborate to establish the feasibility of fusion energy for "the benefit of all mankind" (Braams 2002, p. 250). To the Americans, fusion had clearly received its needed attention in Geneva as the only technical item among Reagan's five points in the summit synopsis presented to Congress was the US-Soviet agreement on major advancement in the field of fusion research, through international cooperation (Fowler 1997, p. 115). His comments included:

> It is becoming increasingly important that we all reach beyond our borders to form partnerships in research enterprises. There are areas of science, such as high energy physics and fusion research, where the cost of the next generation of facilities will be so high that international collaboration among….nations may become a necessity. We welcome opportunities to explore with other nations.... (Orbach 2006)[36]

This led to the signing in 1987 of the ITER Agreement between the United States, the Soviet Union, the EU, and Japan (Braams 2002, p. 250).

I will now, briefly, return to the initial musings of this history synopsis for a slight digression. Indeed, what did trigger off the high stakes set by multiple nations on the decision to launch ITER in 1985, a project within a field of science which technological facets were more intricate, experimental, and cost-demanding than any other comparable venture at the time? On the basis of the analyses hitherto presented, it is my proposal that patterns of path

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36 http://www.er.doe.gov/News_Information/News_Room/2006/ITER/ITER%20Initialing%20Ceremony%20Press%20Remarks.htm – 27.01.2010

dependency might have constituted a significant impact, and that alternative political options of scientific calibre were masked by the strong position of the tokamak bandwagon and its advocates. I claim that the political influence of a few key actors in the fusion community, which put ITER on the agenda, was yet another signal of the momentum rate that the tokamak approach had amassed; ITER would debatably not have happened in the absence of the network of rapidly growing knowledge bases, databases, widespread large-scale tokamak technology, and the weighty interests and positions of various tokamak physicists, engineers, and other proponents present in 1985. In this context, the informal ties within the fusion community are also highlighted:

> With ITER, the many years of Soviet-American cooperation in fusion paid off magnificently. I am sure that this could not have happened without the close personal relationships that had grown through all those years (Fowler 1997, p. 115).

Collectively, these network factors comprised a viable solution to future political challenges, and with an unspoken short-term prestige due to its sheer size. It looks as if attention to large-scale research in general was becoming a literal norm to some – even to the extent of treating size as a desired end in itself. This is still observable:

> International collaborations are increasingly the only way that large-scale scientific facilities can be built…. Therefore, jeopardizing [American] participation [in ITER] is to jeopardize big science…. (Munger 2008)[37]

At any rate, what I do wish to contend more blatantly is that ITER would become the final frontier and main facilitator of the tokamak research lock-in that is vital to the main problem of this thesis. I will elaborate on this in the concluding section.

4.2. Relevance to the dynamic interpretation of Big Science

Having first verified ITER as Big Science consistent with the static interpretation of the term, the investigation of tendencies and events that have contributed to the fusion development that culminated with ITER might provide a valid perspective on the dynamic interpretation. Although it is the fusion field's previous three decades of evolution that invoke the immediate

[37] http://blogs.knoxnews.com/munger/2008/01/field_report_the_iter_misadven_1.html - 29.01.2010

Big Science connotations according to the Weinbergian typology, I find it more accurate to maintain, with reference to Capshew and Rader, that the lifespan of fusion has displayed a complex state of pioneering affairs that have always involved 'bigness'. This is in part signified by the efforts that sit constantly at the frontier of human knowledge to reshape and redefine the premises of energy production, but, above all, it is justified by the fact that fusion consists of a dynamic network of politics, scientific prowess, and innovative research technologies that already in the 1950s established the embryonic stages of a seemingly path-dependent pattern, and thus it is my contention that fusion was to become an inherent agent of growth, seeing as it developed its own momentum over time. At the beginning, the initial movements in the political framework provided important incentives for fusion to serve as an instrument in the Cold War (however primarily peaceful) along with parallel ventures such as thermonuclear weapons research and space programmes. This was a relatively short-term, yet noteworthy contribution to establishment and incumbent progress. With the origin of fusion research internationalisation in 1955, and the following research declassification in 1958, network factors would mix and subsequently multiply from the feedback effects acquired through distribution and exchange of personnel, information, and technology. This became especially visible during the late 1960s, explicitly demonstrated by the Culham expedition. The intimate interplay of country-specific technology and expertise observed with this venture brought the collaborative characteristic of fusion research to a new level, facilitating the breakthrough of the Soviet tokamak. This decisive episode is almost a paradigmatic example of how radical progress may occur when highly critical network factors coalesce. I use the term 'radical' to point up that the 'tokamak revolution' brought about a significant change in the technological focus in fusion: for the first time, a single, dominant approach among the heterogeneous range of existing designs was distinguished, amounting to a critical turn in terms of strategy as it provided a substantial delineation of toroidal magnetic fusion as the most viable concept for the fusion community to pursue. Hence the importance of a dominant technology acting as one of the driving forces in Capshew and Rader's dynamic interpretation of Big Science: the tokamak materialised a concrete, salient feature on which corresponding network resources could be further concentrated and amplified.

The ensuing adoption of tokamak technology worldwide, supported by changes in the political framework, caused a notable expansion, and integration, of network growth factors: with the international scientific community jointly embracing one particular fusion concept, the tokamak approach became interconnected with additional channels of knowledge in new and different places, and these were, in turn, interconnected through the tokamak. Moreover, when assessing the rapid drift towards qualitatively and quantitatively larger prospects of tokamak research, it leaves little doubt that Big Science can be construed as the very process of science growing into massive proportions of, not surprisingly, "money, manpower, machines, media, and the military"[38]. And this process, as I have referred to earlier, is not only one of growth, but one of continued growth in the implicit and explicit sense, i.e. the continued growth that is nourished by momentum, and the *need* for continued growth through an ever more beneficial selection environment after momentum breaches the capacity to meet its demands. The latter is indicative of the Big Science paradox of ITER which I will turn to in the following main analysis section.

4.3. Main analysis

Up to this point, I have made an attempt to define ITER as a genuine Big Science specimen. First in terms of what I have called a static interpretation, typed *Big Science*, which derives from the original Weinbergian conception that focuses on the scale, scope, and significance of science as it is observed post-WWII. This conception is contextualised further in a relative time frame by Price with his mathematical presentation that introduces the idea of exponential growth of science, which in turn leads to my second Big Science definition. Accordingly, I moved on to discuss whether ITER can be understood in view of the dynamic interpretation, typed *big science*, as collective driving forces that make science bigger, which was introduced by Capshew and Rader (with essential reference to Price's work). In order to do this, I made a summary of the documented history of fusion that leads up to the establishment of ITER by structuring key variables that have jointly contributed to the field's progress in one way or another. What is more, these variables debatably compose a path-dependent growth pattern with which I chose to operationalise Capshew and Rader's interpretation. Finally, I will now employ this theoretical basis to analyse how the massive magnitude attained with ITER has subjected the project to several complications.

The official statement is that ITER was established as a prospective solution to future energy demands and as the selected promotion of international commonality through research and economic integration during the concluding years of the Cold War. Politically, ITER's Weinbergian Big Science connotation was likely an additional incentive according to the idea

[38] In the case of fusion and ITER, "military" is chiefly replaced by policies of energy and environment. The important point is that Big Science often implicates compatible and/or complementary ties to government.

of 'big is better' and to the technological optimism that is implicit to the prosperity of fusion. However, the majusculed *Big Science* as a rhetorical construct and the minusculed *big science* as concrete large-scale research activities may have different ramifications that I contend can potentially undermine them both. In the ITER case, this dual undermining is discernible in a reduction of the whole Big Science notion; a reduction that mirrors the actual downscaling and several postponements of ITER's featured technology, the method of toroidal magnetic confinement fusion, represented by the tokamak device. To begin with, the ITER machine, as it is known today, is a fairly concentrated version of the original design that was first proposed in the 1998 'Final Design Report' following 3 years of CDA and 6 years of EDA (see section *Big Science as industrial production*). The report presented an outline which complied with the EDA programmatic objectives, the detailed technical objectives and technical approaches, and the cost target for ITER that had been verified by its participating countries in 1992 prior to the EDA (Summary of the ITER Final Design Report 2001, p. 8). Back then, this was an approximately $10 billion tokamak reactor (Glanz 1998, p. 1) that was well-rooted in the knowledge and figures already produced by smaller, concurrent tokamak experiments. Also, as I have mentioned earlier, prototypes of various components central to the machine, e.g. superconducting magnet coils and sections of the vacuum vessel, had been previously constructed and tested as part of the EDA. The full reactor construction phase was planned to last for a period of around 10 years, succeeded by a twofold operation programme that comprises a 10-year physics phase and a 10-year technology phase, in addition to 5 years of decommissioning. Moreover, the work by the Joint Central Team had been monitored and directed by all ITER partners, maintaining mutual information and thus minimising the risk of unexpected problems. This featured the 1997 Detailed Design Report (a precursor to the Final Design Report) that had been assessed independently by the partners and deemed "technically and scientifically sound" subsequently (Braams 2002, p. 255). In spite of all the favourable indications, the 1998 ITER design was ultimately disapproved and regarded as "defunct" with the project cost being the primary obstacle (Glanz 1998, p. 1).

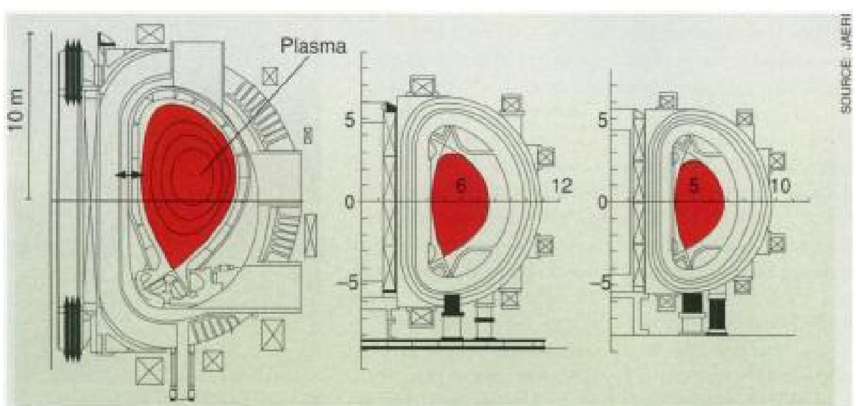
The reasons for this rather abrupt change of course have, as it is stated, "as much to do with events outside fusion as with anything within the programme" (Braams 2002, p. 255). One important aspect is the notable shift of national policies as well as international relations that had occurred since ITER's inception in 1985, with the collapse of the Soviet Union being one of the most influential happenings. Because of this, the political rationale behind ITER as a scientific link for East-West collaboration and economic integration to bring stability to the Cold War was no longer valid, consequently killing the incentive for pursuing an international fusion effort for peace (ibid., p. 256). With regards to funding, the original ITER design was technically affordable, ironic as it may seem. The overall world expenditure on fusion research in 1998 was in the region of $1,4 billion (1989 exchange rate), while the ITER Final Design Report estimated an average of $750 million in annual construction costs and an average of $400 million in subsequent annual operation costs, amounting to a project total of approximately $15,5 billion. However, funding in 1998 was already allocated to existing fusion projects from a worldwide mixture of national budgets and could therefore not be easily diverted; to the (then undecided) ITER host country, even the initial site and infrastructure costs would require a dramatic squeeze on its running fusion programmes. Hence, a significant increase in subsidies was imperative for the ITER partners to even consider the selection of site location and the initiation of machine construction. Furthermore, funding issues transpired due to the fact that the agreement of equal sharing of construction costs among the (then four) partners was no longer viable. This was a result of a continual decline in the American fusion effort and the Russian programme being "but a shadow of what it once had been" (ibid.). There had long remained an expressed reluctance about the ITER project within the American fusion community, at the same time as it featured some of its most fervent advocates, but the majority found the project acceptable only if it allowed parallel research into alternatives to the tokamak or means of making the tokamak more compact. By 1995, however, the American fusion budget had been reduced by 50 percent compared with its real term value in 1977, which demanded shutdown of several alternative research activities besides curtailing the few that remained. This escalating contest between ITER and the basic programme triggered "an erosion of support for ITER" within the American community as some government officials opted for severe polarising (ibid.).

As a result, downscaling would become the only practical solution. Until 1998, Japan (and Europe) had officially rejected the Americans' suggestions of decreasing the size of ITER, but then a 3-year delay in any decision-making by the ITER partners on whether to implement the project was settled following Japan's own economic challenges[39] at this point. During the delay period, a range of next-step fusion experiments were considered, often in the shape of scaled-down versions of the original tokamak in addition to even smaller devices that featured magnet systems built with regular copper coils as a replacement for the "delicate and costly superconductors" (Glanz 1998, p. 1). One approach to downscaling, as proposed by JAERI, involved a lower performance target by reducing the goal of plasma ignition to a

[39] http://www5.cao.go.jp/99/f/kaiko-e/kaiko-e.html - 29.03.2010

power input-output ratio of merely 10 or 20. In comparison, plasma ignition implies an input-output ratio of infinity. This reduction would decrease the overall diameter of the ITER plasma chamber by 30 percent, the lighter strain of fusion heat and radiation would permit a leaner shielding for protecting the cryogenically cooled superconductors, and the full machine support structure would thus be revised to save costs. MIT proposed a similar downscaling approach, only with even leaner shielding. Despite the modesty of these designs, they were regarded as adequate for measuring essential fusion feasibility, but it is clear that the original concept would suffer a significant setback; an input-output ratio of 10-20 equals minimum compliance with the scaling laws and a minimum value of what the ITER reactor ought to accomplish for feasibility testing. Tokamak critics, on the other hand, stressed that the efforts to find a functional way of diminishing the original ITER machine nonetheless relied heavily on the underlying principle of scaling-up previously obtained knowledge rather than pursuing more innovative and cost-effective methods for reaching ignition; a lighter version of ITER would by no means be 'small' in the real sense of the word (see figure 10) (Glanz 1998, p. 1-2).

Figure 10. Cross sections of two schemes for a half-price alternative to the original ITER tokamak (left) (source: Glanz 1998, p. 1)



Eventually, a Special Working Group (SWG) was established by the ITER partners to offer technical guidelines for a satisfactory design substitute (Summary of the ITER Final Design Report 2001, p. 8). After 3 years, in 2001, a fully revised ITER approach was approved – the one that has remained since, and which I have presented in the beginning of this thesis – with a cost reduction of 50 percent from that of the original design.

However, even after downscaling, the nature of ITER's extent has kept a tendency of hesitation and risk aversion among its stakeholders with the occurrence of external changes. For instance, with the merger of Japan's two major science agencies in 2001, speculations on whether the country's other fusion projects would again be put in a more direct competition with ITER for funding began to surface among scientists. With all fusion subsidies distributed from a centralised budget, a 60 percent national share of ITER's overall expenditure could well exclude other fields of research. Members of the Japanese fusion community have often stressed that ITER is merely one of several candidates, arguing that alternative technologies should be pursued as well. The Japanese Large Helical Device, a $650 million installation that confines plasma in a magnetic field generated by spiralling coils (inspired by the stellarator), has been highlighted as one such alternative, albeit having experienced one or two decades shorter maturation time than ITER's tokamak technology (Normile 2001, p. 1). The capacity demands of ITER have even put significant pressure on other tokamak projects such as JET, threatening to shut it down. This is due to the EU's fusion budget being incapable of covering its 13 already existing national fusion programmes while financing the construction of ITER, despite the ITER partners' efforts to minimise costs (Giles 2003, p. 1). Indeed, if we turn our attention to the present, it is clear that the decision to modify the original ITER tokamak into the half-price 2001 version has basically limited the scope of ITER's science without making a real difference to the funding problem. This is owing to "omissions or underestimates" in the 2001 calculations, mounting prices of raw materials such as concrete and steel, and incremental adjustments to the design (Brunsden 2010[40]). Adjustments include making the ITER installation earthquake-proof as the 2005 agreement at Cadarache as project test site involves a risk of harmful seismic activity, a point which was not addressed in the early designs (Sample 2009[41]). Increases in staff numbers following the 2006 formal launch of the ITER Organization are also to be accounted for. Hence, as of 2009, construction costs alone are expected to surpass $16 billion (McGrath 2009[42]). This has made certain member states opt for a revision of the project's timetable which had first planned for a tokamak start-up in 2016, but was later postponed to 2018. European governments in particular, being responsible for covering the majority of both construction and operations spending (45 percent), now regard 2018 as overly ambitious as well. They fear that the budget is "spiralling out of control" and would prefer to alleviate the impact of costs by spreading them over a longer

[40] http://www.europeanvoice.com/article/interest-in-reactor-cools-as-construction-costs-soar/67041.aspx - 28.02.2010
[41] http://www.guardian.co.uk/science/2009/jan/29/nuclear-fusion-power-iter-funding - 27.02.2010
[42] http://news.bbc.co.uk/2/hi/sci/tech/8103557.stm - 27.02.2010

time schedule (Brunsden 2010). The practical implications of the present situation are held to be the EU's request for risk mitigation by building prototypes of two components central to the ITER tokamak before they move on to construct the final versions. The items in question include parts of the vacuum vessel and half of the toroidal field magnet coils. This is incompatible with a 2018 project deadline as it requires the EU to start building the on-site manufacturing facility at Cadarache before completing and testing the prototypes, which in turn would incur costly modifications to the partly built facility should it prove deficient. However, it is also held that further delays are due to EU financial rules which state that officials are prohibited from dealing with contractors if not a budget for the whole contract is assigned. Moreover, EU nuclear research funding is endorsed in 4-year intervals, and the current budget (ending in 2011) does not meet ITER's now-inflated costs. "The EU can not promise money it doesn't have" (Clery 2010, p. 1). Hence, a further deferred construction completion date of November 2019 is currently[43] being assessed.

This reality is a contrast to the initial purpose of magnetic fusion research as presented to the public, Glass underlines:

> The scientific community needs to re-examine the premise on which the public was originally asked to support fusion research, namely, that it would lead to the development of a practical, power-producing technology. In light of today's knowledge, it is highly unlikely that further development of the tokamak will lead to that outcome (Glass 1998, p. 1).

Is it merit in assuming that ITER, hereunder its immense technical design, cost estimates, and organisational nature that will likely require a high degree of centralisation, has advanced into a paradigm inconsistent with the modern industrial world, not to mention the industrial world of tomorrow? It is clear that the coupling of supply-side impulses represented by decades of evolution within the fusion community, i.e. tokamak maturation, and those of the demand-side represented by political interest and the need for a next-generation energy source to replace the use of fossil fuels, has instigated an international science venture of substantial innovation, prestige, and importance. Nevertheless, ITER may currently be just as much considered a resultant system of suboptimal political strategies, being fixed within tokamak parameters and thus unable to adopt approaches to alternative fusion methods should they prove sufficiently viable. Moreover, Fowler deliberates:

[43] March 2010.

ITER is better characterized as the conservative effort of a worldwide community of tokamak scientists and engineers determined that their record of accomplishments, which has seen no real failures over more than twenty years of working at the frontier of knowledge, remains failure-free (Fowler 1997, p. 124).

These circumstances may indicate that the field of tokamak research has turned rigid from path dependency patterns, i.e. lock-in mechanisms, with the signs of this condition being that ITER has eventually become inflexible and unsuited for thorough restructuring. I believe such a suggestion is plausible partly due to the nature of the tokamak machine that is seemingly intrinsically monopolistic, making it prone to unwelcome compromises and modifications; and partly due to the social facilitation of the fusion community and the political framework that has enabled the machine's development, provided its progress, and made science become dependent on it. This is on account of the tokamak's technological preconditions, dominant position, and extended maturation time, in combination with a richly accumulated scientific knowledge base, inertia of sunk costs from industrial and governmental expenditures, and prevalent scientific and political advocacy. In other words, tokamak fusion implicates a semi-autonomous array of driving forces and limitations which, when sustained through ITER as parts of an international Big Science political-scientific strategy, have become obstacles on the way.

This, I hold, is the hands-on divergence between the concepts of static and dynamic Big Science interpretations of ITER. The former signified by the utopian, futuristic vision for a limitless energy source to be reached through worldwide support of the most modern and sophisticated energy science and technology available, coupled with the promotion of peace from the international commonality obtained with such a venture; the latter signified by the nitty-gritty mechanisms that have built fusion science and technology over time, and that have continued to do so even more through the amplified social facilitation of the ITER agreement. This implicates that dynamic variables exogenous to the static *Big Science* notion of ITER – i.e. the rhetorical notion that solely relates to ITER's instrumental and political utility as 'big', 'utopian', 'prestigious', and 'promising' – have eventually began to emerge endogenously in the project from the path-dependent leverage of preceding fusion *big science* activities. Hence, I would claim that the difference between *Big Science* and *big science* have ultimately worked to consolidate the momentum of the tokamak at the same time as causing friction between inflating ITER costs and the macroeconomic challenges and unfavourable political shifts that surround the project, bringing about the actual downscaling of the machine and reducing its capabilities. Paradoxically, estimated ITER costs are still inflating today and still acting as an impediment to other potential fusion approaches due to forced redirecting of funds. This is what I choose to call a *negative research lock-in* in which ITER's Big Science paradox lies: that the continued growth once propelled by momentum has turned into a *need* for continued growth via an exceedingly beneficial selection environment for the reason that ITER is too large to stop and too large to continue with the desired pace unless more subsidies are granted; subsidies that do not exist.

More theoretically, I believe the many complications observed with ITER are pertinent to the ceiling conditions contemplated by Price in his initial work on dynamic Big Science interpretations. Indeed, it should be quite noticeable that the development rate of science, supported by large-scale science projects initiated partly due to political and non-scientific interests as illustrated with fusion and ITER, can not continue in the same exponential mode indefinitely. This is because the actual quantitative magnitude of science eventually will reach levels with which society is unable to keep up. And when society comes dangerously close to fall behind, the ceiling is reached. ITER represents a level which is bordering on that ceiling, and its momentum of growth is therefore closing in on a saturation point towards which it displays tendencies of erratic behaviour. Seeing as ITER has been pared down both physically and conceptually to cut costs, only to generate expenditures that are even higher than what was attempted to avoid in the first place, and thus causing several postponements of machine construction start, it is tempting to bring into play the cybernetic phenomenon of hunting. This might be of help to elucidate why ITER seems to linger on, unable to truly escape its capital-intensive size which is instead compensated for by further and further delays; ITER is a symptom of the growth curve of science oscillating. If ITER had not found itself in this situation, it would most probably have displayed better headway with respect to exogenous restrictions of economic or political shifts. Instead, current findings disclose that the tokamak approach is suffering; suffering along with the ITER project itself and other, possibly better, alternatives to fusion.

Concluding remarks

My goal in this thesis has been to analyse how the science and technology of tokamak fusion have been able to mature and become imperative to the ITER project and fusion research as a whole. In negative equivalence, I have sought to argue how the same science and technology have turned ITER into a significant impediment – both to itself and fusion. The answer to this

I have found to be anything but immediate as its origin can be traced all the way back to the very beginning. Actually, the 60-year-period of fusion research history *is* the answer. I have demonstrated this first by defining the history in terms of three critical variables, namely: 1. the international fusion community, 2. the political framework, and 3. the reactor technology itself. Secondly, I have organised these variables into four phases that range from the 1950s to ITER's establishment in 1985, each of which is concluded by milestone events decisive to the continuation of fusion research. Thirdly, by concentrating on the problem of size, I have made use of the theoretical discussion on Big Science to frame the three variables as inherent mechanisms of growth that have collectively facilitated scientific progress and, questionably, ITER's current situation. The latter point is pertaining to the merger of ITER as a policy of *Big Science* and ITER as a result of *big science* as growth. Finally, the growth mechanisms' actual functions and interrelatedness have been made clear with the help of perspectives on path dependency, deriving from evolutionary economics.

While the history of fusion is cleverly presented in several publications, it has not been structured nor interpreted as presented in this thesis. I have used history as a means to give a comprehensive explanation on ITER's background and recent development, and to point out more generally that selected parts of the Big Science world may be increasingly assuming proportions that can pose a threat to own sustainability. Furthermore, I am arguing that this is a plausible consequence of dynamics intrinsic to certain cases of scientific progress; dynamics which I find significantly comparable to evolutionary innovation processes that are normally being studied concerning the evolution of markets and large technological systems. Hence, it is my proposal that the evolution of science is applying to similar growth patterns that can make science turn into a de facto system of its own. This is evident seeing as when science becomes gradually more contingent on advanced technologies to succeed, it can also become progressively more autonomous because the network factors that propel its development are inclined to multiply from the additional components of people, money, regulation, and politics that follow techno-quantitative leaps. And for every leap, these components will reciprocally work towards a new one. When this feedback loop attains a certain mass, I believe the system of science is gaining a momentum that should be taken into consideration in terms of research policy: such an attribute of Big Science may potentially be an asset to scientific investments, e.g. international projects, but also a risk of getting locked into irreversible complications.

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